Schenectady 2 Jan. 1843.

Dear and respected friend

It was with no little delight that I received your valued favour of the 27th ult. and I hasten to copy the passages to which you refer of Fries, which I never valued before so much as I do now. I hope you will find it convenient to give me similar tastes again: I shall be delighted to do all that is in my power.

At p. 254 of the 2d. edition of the Novitia in the remarks upon Hieracium vulgatum Fr. (by the syn. "H. sylvaticum Fl. Dan. non L., Willd., Wahl". — & also "H. murorum, a, L. ex parte.") it is said as follows: — "Pro generis more valde mutabilis ××× In alpinis aliisque locis sterilibus humilior, minus multiflora: Grönlandica optima hujus habeo specimina, sed valde hirsuta, flores majores et calyces hirti nigrescunt. Illi sese prorsus associat H. nigrescens, Willd! Spreng. Syst. 3. 640." At p. 261 is Hieracium prenanthoides, Vill., & Wahl., said to grow also "in Islandia! Grönlandia!" In the remarks on this the author says "Foliorum forma cordata ovato-oblonga lanceolata & linqulata semper vero acuta ××× in Succicis tenuia subtus glaucescentia pulchre ciliata; in aliis v. c. Grönlandicis firmiora non glauca undique hirta. Infima in petiol. amplexicaul. tamen atten." This is all there is in the Novitia or the 1st & 2d Mantissa, the third I regret I have not.

Besides Meyer on the plants of Labrador I cannot finding anything to offer you: but it must be very rarely that you have occasion for any work that you have not. I am sorry that the plants have not reached you before this: but the truth is I was afraid or loth to trust them to the Carrier — perhaps still more truly I did not take pains enough to ascertain that Harnden's truly excellent express was estab[lished] between Albany & Boston

and I was not quite so alive to the importance of your seeing them; especially as I had seen your notes scattered through the coll^n (I believe I am right) and supposed you had made an examination of the whole.

I have been very hard at work ever since I have been here. Mr. Pearson the Prof. of Chem. & Miner. who gives the lectures in Botany is a very active fellow - wishes & we are making an Herbarium for the College; which will I hope some day attain the distinction (when such another work as D^r Beck's — that is I mean not a general work like his, but a work published in this neighbourhood shall be written) of being quoted! I am able to furnish such plants as are most wanted & puzzle so many students — the Cenastia, Elatine, Callitriche, &c and there will be authentick specimens of perhaps all the common British species &c. Our Museum here is now very respectable in Zoology & Miner^y — I have the immediate care of all the coll^ns. They were kind enough to offer me the whole of the Botanical Department; but being sure that I could be useful in this position, I could not think of going beyond it. As Curator and Collector I can do a great deal and shall certainly do all that I can.

Excuse me that I speak so much of myself. But I must tell you a little about some literary labour upon which I have been employed. Being forced to do without asking questions I have endeavoured by myself to make out a number of doubtful plants of my own collecting — and have made a small article which it would much gratify me to publish in Silliman's Journal. As I believe this part is in your hands I thought I would mention it to you. The Article is in 8 sheets of common square letter paper about 11 inches by 6. My being alone here must be my excuse for venturing to publish conclusions on doubtful plants without laying them before those whose opinion might save me from many errors and make the results infinitely more valuable — but I assure you I have never studied any plants before as I have these; and I hope and feel confident that the article will not meet your disapprobation. The subjects are mostly White Mountain plants. Among them were two, three Compositæ, which I of course submit as they are to you. The first is as follows "S. humilis (Herb. Banks) Tor. & Gr. Fl. 2. 206. — var. γ. alpinus (mihi) nana caule 3/4 poll. — 5 poll., 1-8 floro. — S. Virgaurea β. alpina, Bigel. Fl. Bost., Tor. Catal. My. 1840. p. 145, Tor. & Gr. Amer. Pl. 2. 207. — Hab. White Mountains; (a) Rocky hills of the Notch; also throughout the Notch in the gravelly bed of the Saco, brought down by the Spring freshets and becoming luxuriant. (γ.) Stony alpine moors with Potentilla minima β. Torr. & Gr. l.c." There is then an account of those variations which we found. I gathered this plant in 1840, and gave some away in Europe without name. They were accidentally sent out to me while there with Arnica — but I did not distribute either but very little. D^r Klotzsch has both. Then there is an account of S. thyrsoidea merely because it is the only other alpine species — and after that an account of our Arnica of which I felt a little proud — but I fear from what you say that it will not be confirmed. My account is this. Having Pursh's two species I could not but study them too. Please to consider me as having really laboured upon these things, and feeling therefore as if I might advance my views, especially as they are plants of my own gathering. A. plantaginea Pl! referred to the variety angustifolia by Brown Hooker and Meyer I believe — I am sure you will consider, (if it be not a good sp.) to belong not to this variety but to a, the real A. montana. Then if A fulgens Pl! is referred to this variety by such authors as those quoted — how could I think otherwise than that our A. lanceolata (or mollis as you remark) agreeing as it seems to me perfectly in habit (as comp. with the developed 2-foot 3-headed Notch form of our Arnica found by me) and indistinguishable (sit venia!) in the heads from β — Then that this should form a third variety? But I am sure you will kindly consider my Notices, which yet I ought to publish with some confidence. My notice reads "Arnica montana, (L.) foliis ovatis integris caulinis geminis oppositis. Willd. — A plantaginea, Pursh! Fl. 2. 527. — β. angustifolia (Hook.) A. fulgens, Pl! l.c. — γ. lanceolata (mihi) foliis lationibus sub-membranaceis denticulatis. A. montana, Tor. Catal. 1840 p. 148., Oakes Pl. N. Eng. in Hov. Mag. 1841. p. 6., A. lanceolata, Nutt. in Trans. Amer. Phil. Soc. Hab. (a.) Labrador." &c. The Achenia of A. plantaginea I found as large and as hirsute as in A. montana from France. In β. & γ. smaller & less hirsute. It seems to me the heads of flowers are exceedingly similar in β & γ. But I shall be anxious to hear or see your final decision upon A. fulgens: which it seems to me will also decide our White Mountain plants. Finding however that A. montana was consid^d variable, and knowing how well those distinguished authors must have been acquainted with it, I supposed they could not have overlooked the apparent differences.

I have also studied the Betulas — and make four forms on the White Mountains: B. nana, B. Bottiana*! (the large-leaved plant like nana) B. papyracea β. minor, and (perhaps) B. glandulosa, of which I have a beautiful Canada Spec^n ex herb. Lamb. Oakes will laugh at it. But I am sure I have known all the plants for some time well — & I have studied all my collections in 5 years during which I have vis. the White Mountains.

* I was afraid you would abolish "Nabalus Bottii" — I have beautiful specimens of it coll. in 1840.

But I must not trouble you with all this. I hope the article will not meet with your censure in any respect, & I feel pretty confident: for I find that thorough and sufficient labour leads to this confidence — perhaps even to too much of it. My Cambridge Catalogue I hope to be able to write off this winter: at least, at the first moment that my time is free.

This letter would have gone by yesterday's mail, but I had to write the who's over. And even now I cannot send it without asking you to excuse all its errors of fact & manner. I would not mention this if I did not feel it to be necessary. Meaning right, and not often expressing anything but what comes from within — a person may yet by a mistake which should not be censured too harshly, be misapprehended — where too he cannot well set himself

To
Prof: Gray
Harvard University
Cambridge
Mass.

Box sent to Albany 3d. by Harnden & Co 1843.

right. If you then see too much of confidence or anything of the kind, please to excuse it. The heart is not the sinner always where the lips or the pen offends.

Besides the Lambert coll[n] in the Cabinet — I have a large quantity of Pursh's plants viel. Amentaceæ, Juncei, Carex, &c. (The latter hardly worth looking at. Commonly, 2-6 species to a name. But I shall not disturb their position.

I am dear sir most sincerely and respect[y] yrs. Ed. Tuckerman jr.

Prof. Gray.

I have kept one Spm. for last
Park fulfilled to insert the last M. Bicuda
P. S. Please not to credit me with this. If you will
only read it, it is all it deserves.

And Feb. 13,

M. Schen[r] 1. Feb. 1843

Dear Sir

Four pages I had filled in answer to your very kind letter of the 23[d] — the same night that I received it — but the next morning I found I had left out so much that I wished to say, and had said so much (as I fear I always do) that you would not wish to be troubled with — that I deferred and deferred till now. I hope you will excuse this in one who does not profess to be expert at what is really an art — letter writing.

I have nothing to do but say Amen to your decision in regard to my two Compositæ. I was led accidentally to the subject, because two were White Mountⁿ plants. But pardon me if I wonder at your giving up the A. fulgens. It really seemed to me to differ from A. montana, much more than A. plantaginea does. But I stop. I was more sanguine about the Solidago for I thought this was

also your opinion. For one thing I am glad — the settled name of the Flora is not to be altered. This is everything to local botanists: and though they may venture to differ in regard to one of their own plants sometimes — still their Flora is to them their Code and Statute Book.

Of course I cancel that page of my article as you advise. But on receipt of your kind intimation I immediately wrote to Sill. Jour. and offered my Article for the next number. It contains the Small-leaf'd Betula (B. pumila L. = B. glandulosa Sullic., B. glandulosa Mx., B. Bootiana mihi = the large B. nana of the White Mountains — I have a large set of specimens, having long believed there were 2. — B. nana, and B. papyracea B. minor mihi.) Having an orig[l] Mafson Canada spec[n] of B. glandulosa, I feel pretty confident as to all my names. I have not Spach — but the way you treated him in Onagraceæ in the Flora, led me to think that I need not so much regret it. Then follow the Alders of N. E., the White Mountain Grasses, and some remarks on those plants of Pursh which he (as it appears) erroneously credited to the mountains of N. E. I have left out about half of my original article. I hope you will succeed in procuring a person to collect on the Rocky Mtn. Expedition (I suppose) of M[r]. Audubon, mentioned in the Newspapers. Not every man could undertake such a thing. For my part I hope only to get to Katahdin. I often think of it and look forward to the next Summer as if it was possible then. Do you continue to think of the undertaking?

I am very glad that you found anything in 2[d] Box. Please to name the Lamb[n] if not right. The Catalogue is done [illegible] ceæ. I do not know the precise habitat of that Aster albus. I got it of a botanist, M[r]. Eddy, but I supposed it was on the ticket.

I have a few wretched Helianthi of Pursh's, (one is marked "H. atrorubens") which I have often regretted that I did not know of when I sent before. I only mention them now because they are the only other Lambertian Compositæ that I have. These specimens are loose and were never mounted by M[r]. Lambert.

I have not a single specimen of Carex Fraseri in my rich collection. It is so indeed. I shall be delighted to see it and study it, delighted indeed.
I have a head of Cassini — if you would value it I shall be delighted to send it.
I am ashamed that I have but just thought to acknowledge the invaluable Colden Correspondence. Dr. Potter was greatly interested in it as he was is on a life of Colden.

How remarkable a man he seems to have been. And how it will interest Sir Wm & Lady Hooker! I have just read his Memoir of New England Botanists as reputd. in Sill. Journal for 1825. He does not more than mention Colden. I thought you would like the Cassini print.
It seems as if it ought to make the journey to you. It is a small print but good. I forgot it when I sent.
I am dear Sir most sincerely & respectfully
Ed. Tuckerman Jr.

P. S. I do not know whether my right to the plants is quite as good as yours; but I think I must have the satisfaction of seeing them in my Herb., for some time to come. I know their value and shall take good care of them, considering myself rather as a Trustee than anything else. Oakes would say perhaps that I had no business with them at all — but I want to see them in their old cabinet again. I am glad you like the Cassini — & will send it by the next opp^y. Glad too I am that you have procured for Oakes just the work he would think, like it was an old friend of his. And delighted that the new Flora is ready. When it will reach this place if ever, I cannot guess. I dont believe there is a subscriber in the county. But the College will have to subscribe soon. Do you never forget the Mr. when writing? I believe you did at the White Mtns. But excuse all this from your most sincere & respectful friend E Tuckerman.

(U. C. Schenectady 17. Feb. 1843.

(Dear Sir

3

Your letter of the 13th has made me. I need never say how delighted I shall be indeed happy if you go too to Katahdin — and Oakes is everything and more — he will keep us laughing the whole way. But I think we should be there by the 12th. July — it is too late to go. My calculation is to be on the spot, if permitted, early. The Bicknellia flower & Diapensia & Rhododendron the last week in June & the first of July. Having been on the Wh Mtns in every month from June — to September — & knowing how short the alpine seasons are — & that a whole set of plants are through their work and at rest even in the 3d week of July — it seems best to be sure. Should we not arrange to stay a good while: may get there by the beginning of the 2d week of July — and be pressed not to leave till we are through? But all will be settled in time. I can promise you, if prospered, that I shall be a far better fellow traveller than I fear you found me in September: and my winter's studies will not have been lost in a botanical point of view. My article I sincerely hope is in the printers' hands now — this was promised by the Editor, and I depend upon it. Indeed I shall offer it to Mr. Hovey or somebody else if they now disappoint me. I was very much obliged by your kind recollection of me in sending Mr. Spach's Monograph: it begins (this number) with A. glutinosa. I cannot satisfy myself that I need alter anything in my art: when I only touched upon the rest of the species because they illustrated my White Mtn. plants. I am sure our B. nana is identical with

(Answ. April 14.

Swedish and Scotch plants: W. Spruce refers the B. nana of Mx and Hook. to his B. Michauxii. All I mean is that I studied the plants as well as I could — and content with not having opened a rash opinion — I await quietly the revision of the Flora. B. Littelliana is neither of the new species of Spach I am sure — it is possibly included in one of the old: which I suppose depends entirely on the question whether the plant was in the 3 great Herbaria of Paris. B. Littelliana is what I before called B. Bootiana. This cannot be B. Michauxii, not only from the other characters of that species, but also from the remarkable peculiarity of that species in having strobiles from 4 to 8 inches long! "Strobili 4-8 pollices longi" while the leaves are "3-6 lineas longæ" — I cannot help noticing this statement. From B. rotundifolia Sp. it (our plant) is distinguished by the char. of the ♀ in each the former is placed "strobilis brevibus" and the Specifick char. "strobilis ovatibus," ours being = oblongo-cylindraceis. Spach mentions a larger form of B. nana, as does Lin. Fl. Lapp. cum Fr. To this I suppose my plant might possibly be considered to belong — but I want to propose it as a distinct species, believing it to be one: subject to the final Screw of the Flora.

It does not seem important for me to change the name of A. crispa in my descr. to viridis to which Spach asserts the Mich. plant — and also a plant of Nuttalls (marked! — I cannot make any other guess than that his penetrating eye detected it at the M. Mts., though I thought before that nobody had it before me from there — except that Cutler might have sent Muhlenberg his specimen,) though he takes no notice of Britton who uses the orig. source, and does not seem to have consulted Willd. or Pl., the former having no A. crispa nor the latter any A. undulata as cited by him. I always thought these things were not to be overlooked in monographs — & I hope you will excuse me in this writing just as I think in a tone perhaps not respectful to the learned botanist — but nevertheless (good or bad) really mine. I do not Kochs arrangement of A. viridis (Alno-Betula) viridis for preferable to making a new genus for what is & will be always an Alder. I do not know enough to judge correctly of the generick characters (that is what is important enough & what not) of hardly any plants — but I am sure for my own part I should follow Koch. I find Spach has referred Pl's crispa to incana (this must have come fr. Nutt.?) but I had also on the auth" of his orig. specimen for "Pokono". In my art. I have adopted the name of Marshall for our A. serrulata. I am confident this will be done at some time or other; and why if satisfied I am right should I wait. The name of your own botanist is not only the oldest but appropriate — very much so — while the other is without any meaning. In naming it I think Aiton must have had the common English alder A. glutinosa in mind — the leaves being less strikingly serrate — but there is not either of our other species which is not much more strikingly serrate than A. serrulata. Why should an American name give place to a later, bad foreign name? And who can mistake the descr. of Marshall? — My art. is not of much importance in any way — & may be called an exercise in Phytography as Sprengel calls it. Next must come Phytotomy! or you kindly suggested — though no one who can tolerably describe a plant is absolutely unacquainted with its structure — but that is a vast and great field! I am content with my article, and shall be with your treatment of it in the Flora, be it what it may.

But I am going to astonish you with a real work. I have been now 3 months trying to put my Carices into their place in the Herbarium, and what is the result! A complete method (suffer me to magnify my humble labours, for to whose opinion shall I look if not to yours!) of 300 species of Carex — & thereby a view or sketch of the whole genus!!?! it is all in Latin and I have this day finished a preface of 2 pages! in which I have got through quite tolerably I think. The confession that I found in anctoribus celeberrimis was wonderful — is not the Stigmatick arrangement of Carex the last stronghold of the artificial System? Kunth places some species marvellously. He takes C. vaginata from panicea — and C. rotundata fr. ampullacea & then puts them both with binervis! But I must stop. I hope to send you the thing in print in a few days. I have tried to find the natural place 1st of the species — & then joined them into families or groups — 2d of the groups. Believe me I have laboured intensely and I am sure if one sees what Boott — how imperfect an arrangement he gives — putting pedunculata with Mertensii H K — he will hardly blame me for my attempt. My general (I say my, for wherever this arrangement is the same with that of others it is still the original result of my study of the plants and is entitled to that value) divisions are those of Koch — but I venture to alter considerably. According to him the 1st section is "Psyllophora" excl. from dioica to paniculata — the 2d Vignea from the 3d Carices Legitimæ. My view is this 1. Psyllophoræ as above 2d Vigneæ including Kochs section Cyperoides — my Vigneæ begin with Ovales and end with Paniculatæ. 3d Sub-S Vigneastres ED — to include the E. Indian paniculato groups of which I have 5 representatives — 4th Leptantheræ ED. to include the Canescentes, Elongatæ, Tamuliformes Baldensis, Stenophyllæ, incurvæ and Lagopinæ. 5th Carices Legitimæ beginning with the acutæ and ending with Phyllostachyæ — the centre form being C. Pseudocyperus — this last remet being wholly unexpected I was pleased with. If these were shells we should think nothing of a conchologist setting to work to arrange his coll.n to suit himself and then publish his arrangement: — but it seems more in Botany — & I feel as if many would say — "What has he to do with this?" & so on. But I hope it will much soften the way for the next arranger. I do not think a professional man, however patient could have given the labour that this has cost. I believe now I could name any European Carex, not Rupsian or Mediterranean. I have more than 300 — though there are many American I have not seen such as venusta, dæycarpa &c. About the Psyllophoræ I am in doubt. It seems to me that all the dioicæ are but forms of the Stellulatæ including also C. Spicata Schk., and should be consid. as exterior forms of the above. So I think paniculata & microglochin are only outside forms of the Lupulinæ — though C. Collinsii or tubulata. C. eurosina seems to me to belong to Bicolor, if not identical — next to it. (I have beautiful specimens fr. Vahl.) There remain then in Psyllophoræ only the rest of the Scirpoideæ — viz ~~Hindi~~ Scirpoidea capitata rupestris, filifolia obtusata, & nardina Fr (Hepburnii Boott) — and the Pulicares — pulicaris Pyrenaica and nigrescens — to be considered — Are these also to be reckoned forms of other more perfect types? But all this as an afterthought. In my Art., the Psyllophoræ stand as left by Koch. Allow me to give you a single

specimen of the preface. After mentioning the authors I say "Dicat forsan lector Carices nunc prorsus exquisitas optimeque digestas esse. Miror dum nego. Species quot multæ aliarum, et pure temporis filiæ! E. formis transitoriis atypicis, e speciminibus immo pessimis nonne multæ originem duxerunt? ×××××× Primum et ultimum in Botanicis dixit Linnæus Methodum Naturalem. Quis dicet Carices naturaliter dispositas?" etc. The part further on is much better. I have attempted all this 1st — as the only way to see my own collection of Carices in order — 2d to please my fancy in the Camb.e Catal.; and I am having it printed in order merely to gratify myself. Please to keep it a secret. It is ded.to to Dr. Torrey to whose letters, specimens and works I have owed everything. I do not think of publishing it: but have it printed at my own expense. and see what you & Dr. Torrey & all think. In most of this arrangement, which is the third that I have this winter constructed of my Carices the series of groups or chain seems continuous, but here and there (that is in 2 places) the series seems to be interrupted. Whether this be an evidence of errors in the method or a blank which species that I have not got fill up I do not know. I can go no further and can work no longer on it: the labour, to me at least, and I have some perseverance has been very fatiguing; and I rejoiced when after a weeks rest I could see nothing to alter in my list. But I am sure that the place of many of your species is determined. But as usual my letter is all of myself — and I have omitted a thousand things. Your kind offers as well as that of Dr. Torrey I am greatly obliged & grateful for. But I have had work enough: and dont myself mean to do any more at present. If I can be of any sort of use to you I shall be most happy. When you send the plants please see that the men are obliged to take care of them; they are very careless. I have some materials in Pursh's MSS — but I am tired of all work; and have the College and my own coll.n to arrange — not yet done — & to be before spring.

Gray Herbarium
Cambridge
Mass.

He noticed Plantago major & other things. Then Cutler. 1785 — who is cred. with 380 plants. Then Bigel. 1814, 1824, 1840, Oakes, Greene &c. But the moment it is quite ready I will send it to you.

Excuse errors in [illegible]

Yours truly
Ed. Tuckerman

Mt. Coll. Schenectady. NY.
9 May 1843

4

rejoined May 20.

My dear friend

I was again delighted to recieve your letter of the 29th ult. I have just returned from Boston! I went on in great haste, on account of my father's being sick — having mised a letter which was to have reached me and which informed me of his being much better. The time was short, and as I carried nothing with me but my clothes, I had to hurry back. I shoned delight to say a little of my Carex pamphlet. I have thought over all your remarks. I don't know what else to do with C. flexilis. I have plenty of fine specs — but only one of C. Cherokeensis: that however a very good one, & from Dewey. If my C. Cherokeensis is the real thing — then I dont see how I am wrong. C. pedunculata I accidentally omitted in the Enum. though its place is indicated in the Preface. I am the first I believe who has settled its true place, with the Digitatæ — & nearest to digitata & clandestina. It is singular that Boott, familiar with C. digitata & no doubt also with the attenuated base of the perigyn. which beautifully separates this, gynobasis, & the allies from the allied groups — sh'd yet have placed C. pedunculata — when? — between C. Mertensii & C. Franklinii! Kunth is as singular in his view. He makes a group of this and CC. Willdenowii, Steudelii and nigromarginata! You (ignoscat lector benevolus) put it with ovata — the stigmatical arrangement causing a natural position impossible.

Dr. Boott says C. Macenzii, Carey MSS. is C. marginata. So it seems to me.

But you see how I go on about these things! But let me say a word or two more. The groups marked Fr., Kith., & so forth are sometimes really mine. Fries indicated a group of 3 species to wch he gave the name Montana — but the group of my Enumeration has 23 species.

As to the characters of the groups: I had prepared them for the Vigneæ — derived wholly from the perigynium, and only stopped, because I was tired — and didnt see any use in it. The group is associated first by a view of all the characters — then 2dly the task is to set down and express this or that general summary of agreement in one character, as the fruit. Thus the Vulpinæ, Muhlenbergianæ and Arenariæ are all distinguishable by the perigynia. Again it seemed to me that in these sections of sections of sections of a genus a character was less necessary: and perhaps would not be used. Everyone admits that Careyanæ are a distinct group, however near to Digitatæ on the one hand and Phyllostachyæ on the other. I have had a very pleasant letter from the excellent Dr. Sartwell — he is pleased with my arrangement — but actually says that he cant read the "Annotationes" for want of knowing Latin. This is melancholy! I shall write him that if he does not, it is time he did, what every gardiner in Scotland has done. I do not believe there is any language so easy as the Latin — it is the received classical language of botany — and will be while there are botanists — for who knows Swedish and Danish — not to speak of Russian Turkish &c? Hence it will never be obsolete. As Colden says it is not only easier to write in, but far more elegant and precise than English, for this purpose. I am not sure that I ought to express any gratitude for your revision of my article, for you would have done it whether I was thankful or not. But I am sure I appreciate all, and feel your kindness in writing to tell me about it. But it is rather too bad to treat that poor little Poa so cruelly. You leave me in a mare, among all these dilemmas: 1. it is no Poa; 2. the name uniflora will do for a Poa! (cf. 1. char. of the genus; 2. char. of the species — never less than 2 flowered, nor can be that I see (excuse my zeal in stating what I can) and 3. that it is some plant or other already described — but what! But you must bear with me. I know that you have more to do than to think of my Poa long — & as you told me, you will postpone it till the subject is brought fairly and wholly before you. So I wait in hope. At least, was anything known of Muhlenberg's "Poa? uniflora" till I made this known? And so with the Lycopodium. I studied it very carefully. I saw Mr. Spring's article, (I was a little acquainted with him in Paris — and saw him elaborating his Lycopodia. It was on account of the char. by wch he distinguishes his variety of L. annotinum that I did not insert it: supposing as I did this character to belong to the species, as given by Wallroth.) But I studied all the plants mentd in this article as carefully as I could: and look forward therefore with entire content to whatever be the final judgement of the Flora.

My Cambridge Catalogue is now ready. Let it be my work. I make it only as a Prodromus of the Flora. But I want to be the publisher of one separate opuscle on our region. This shall be mine alone, and has nothing to do with that Manual which every one would expect you to be at the head of, whoever else was concerned in it. That will be

all the better for this prodromus. Title – "Enumeratio Pl. agri Cantabrigiensis ab Ejus" The Ager Cantabr. is identical with the ager Bostoniensis. but the former name seems more proper. The region is indicated in the 1st edit. of the Fl. Bost. 1814 and embraces a circuit of 30 miles from the Old State house: a granite greywacke & porphyry diluvial region – with neither the mtns of Berkshire, nor the warm sandy swamps of the South Shore. It goes to within 6 miles of Plymouth, without taking a single one of the Plymouth rarities – the whole North of Plymouth co. belonging to us, but the Southern portion to the Flora australis or meridionalis of the Commons. But

Jo. Prof. Gray
Harvard University
Cambridge
Mass. U.S.

Single. Paid.

to balance this we have Gloucester & its Magnolia; Dedham and its Rhododendr. max.; Newksbury and its Grecian rarities; and all Essex, [redacted] redolent of oakes. But I long to lay it all before you. There are one or two new notions – such as indicating the date when every plant in the Flora became scientifically known and the publisher or finder's name. Joselyn is the first of our authors (Vide Rarities Lond. 1672., & Voyages Lond. 1675.)

Boston 16 Dec. 1843.

My dear friend

I owe you many many apologies for my long delay in answering your last letter — & my long postponement of my visit to Cambridge. I have been looking forward to it constantly — but two very strong reasons have prevented — 1st repeated attacks of Rheumatism which now keeps me at home & 2d my very anxious desire that you should not see my Herb^m before it was arranged. While at Schen^d I managed to put all the new plants into the Orders — but got no further. Of course I could find nothing if I wanted it. Now however I have been working steadily about a month & hope soon to be through — having reached Onagraceæ in the Alphabetical order. The large Orders Compositæ Leguminosæ &c I put into the Tribes as given by Endlicher. In some Orders I have a great many species: & some have almost all the important Genera. I have 52 species of Astragalus & 16 of Oxytropis. I long to have it through, and then to shew it to you. I am in trepidation now the whole time lest somebody should come in before the time. It will look very neat.

W. Oakes & I start very late at the Mt. Mts

As I went on the last day of August I did not expect to gain anything but health — but I found Subularia in the Franconia pond — Aspid. aculeatum at the Wh. Mts, an abund[t] station — Rhinanthus Crista Galli in situ naturali in the alp[e] region of Oakes's Gulf (the 1st time & very singular that so conspicuous a plant sh[d] have escaped botanists) as well as Luzula arcuata (I cant see why it is not also L. hyperborea? but this is rash) & Festuca vivipara — both also new — but Oakes found the latter two before me the present season. Of Lichens & Mosses we collected a great store. The latter Oakes means to attend to. The former I have already made into a Catalogue of the White Mtn Lichens — ± 148 species — 38 of which are wholly new to our Flora & 50 have only been put here in my Articles in Jour Bost. I longed to get through this & so worked steadily & with my large coll[n] of Lichens managed to get through. There are a great many rare & remarkable Lichens but a deficiency of the common & well known forms which I have neglected because already familiar.

Sir Wm Hooker has written me a very kind letter noticing my Art. in Silliman & mentioning his wish to have the supposed new species. Dr. Booth has also written 4 whole pages about Carex. He says C. Torreyi of my Pamphlet is "a good species". That it is wholly diff[t] from C. pallescens & belongs to a diff[t] group of species I was sure — but could not believe that so distinct a plant could have been passed over

Having studied it again I am satisfied that it belongs to the Montanae & is near C. Supina! I have also studied C. Fraseri & put it in my Enum. in Leptantherae, Sub § Baldenses. It seems to me to go with C. Baldensis — In this case I would call the group Fraserianae. But perhaps the fruit which I have hardly seen enough, will separate it as the type of a distinct group — next however to Baldensis. I was delighted when I reached a conclusion on this splendid species, to which I had so long been a stranger — & yet knowing it by name.

But I meant when I first sate down to say how glad I was with all our Botanists to know that you had returned in safety from the perilous South. Dr. Torrey mentioned in a letter that your absence was protracted longer than he expected — & he feared that you might have been seized with sickness. What a Garden we shall have! Poor old Carter must be frightened at the sight. His Dahlias I hope have vanished & their place is better filled. But how much I would say — & how much I long to see. Let me only finish my Herb[m] & I trust I shall see dear Cambridge once more and once more — that Sanctum upon whose threshold I have hardly stepped.

Excuse this too hurried letter — but I will bring my excuses altogether as soon as I can possibly — and till then I am

Most sincerely & respectfully yours

Ed. Tuckerman

PAID

To

Prof. Gray

Cambridge

Mass.

single paid

If not too much trouble please to mark such plants as you think requiring it. I see some are already noted — both by you, & Dr Torrey. This was one reason why I did not send them: I concluded you had exam^d them & made your notes. But of course you would want also the plants. Am I wrong about Betula pumila? I was quite confident, but the figure in your Textbook leads me to doubt. But then is a ? there too. I know Betula glandulosa by an original Canada spec^n of Mafson — & the Ohio plant is not that, while it well accords with the descr. of B. pumila, & I cant help thinking it the plant seen by Kalm in Bartram's Iter. 1. 108.

I have made up my mind to prepare a Century of Lich. Nov. Aug. Exsicc. There are some fine things: and my names are right: besides, which I shall send all my copies for distribution is all I intend — Mr. Greene (Boston 28 Jan. 1844. plants to Fries. A few days, nothing will be more satisfactory to Correspondents. I hope, if favored, to complete my stock of specimens, at the White Hills next summer — Oakes, & I hope to spend there!

My dear friend

You will have time to read this I know. It is a part of what I want to say — but could not express half as well in conversation. 1st. The Juncí. I will tell you what stopped me — finally — in my present study of the genus. Meyer has a remark under his J. paradoxus that under the American J. nodosus of L., many species lurk: — as from the European J. articulatus, L. a whole group of species has already sprung to light. Here is the sticking point with me. There are not enough specimens of hardly one of the nodosus group — either in Herb. Torr., or my own collection — to pretend to determine. Nor have I enough of the corresp^g European series (differing as my specimens do of some species very much from each other) to make up my mind — whether, as Hook & Spreng have begun to do — we should not locate the European series in our Flora — and call our plants acutiflorus (as Hook) or lampocarpus (as Spreng.) — or on the other hand consider ours as a distinct analogous series. I have one specimen of a Cambridge Juncus (aff. polycephali Michx. & scirpoidi Lam.) almost perfectly like one of my specimens of J. lamprocarpus — but not like the others. — Have I not put the difficulty in a clear light? Am I not right in concluding that more study of the living plants and large sets of specimens — such as I have seen in no collection — are essential to the unravelling of our Juncí — which do not need names so much as a masterly revision — which if done in the most thorough^{way} should also settle the foreign species. — How generally does this genus and the exceedingly near Luzula seem to have been neglected! Meyer is allowed to stand alone. — Hooker's J. acutiflorus a, (B. is exactly our J. acuminatus and of Mx. fide herb. Torr.) is (fide syn.) J. pelocarpus, Mey., orig^l descr. from Mag^s specimens. Hooker says he cannot distinguish it from the Europ^n J. acutiflorus — but not only will not Kunth's descr. that of Hooker at all answer to his plants — but I am persuaded the J. pelocarpus is a very distinct species: though hardly known in this country. It grows at Burlington Vt. — & is probably incl. by W^m Oakes in his J. acuminatus — but I am sure it is very distinct — & will find a place in our Floras hereafter. It is not in Herb. Torr. Hooker's specimens are fr. Arctick America. — J. dichotomus Torr. herb.! Gray! Oakes! is a diff^t plant from Elliott's in many respects — but partic^y in its nodulous leaves, always apparent on dissection at least, — its acuminate-rostrate fruit &c. Elliott's pl. belonged to the flat-leaved section of J. tenuis &c. — & a sp. of this section cert^y is ref. to it both by Meyer & Kunth. Our plant agrees with this latter section in its solitary flowers — perhaps more properly in this case we might say one-flowered capitules — but in habit leaves &c it is clearly a member of the other section. I have ventured to call this J. Conradi because in Herb. Torr. I find some spec^{ns} ticketed J. viviparus Conrad — which viviparous state is very common in this sp. I dont know however whether Conrad put any descr. (in Trans. Acad. Philad.) or whether if he did his name must be not be retained, however erroneous: and asked this question of Dr. Torrey in sending him the plant — which I called J. Conradi only to denote that I thought it was a distinct species. I sent a descr. with it. Could you give me a word of advice as to this. It being a Plymouth pl. of which I have a stock of spec^{ns} — I am the more desirous to

have it settled – particularly for one reason. Though some inadvertence Prof. Kunth told me unhesitatingly when I shew this to him – that it was Meyer's J. pelocarpus – and ever since I have dist. it under this name – ~~with out~~ without looking at his descr. – which is ludicrously inapplicable. When however Sir Wm. Hooker gave me his J. acutiflorus which Meyer himself pronounced his J. pelocarpus I found ~~that~~ that my Plymouth pl. must be studied & fixed: whether as J. dichotomus (which I can't believe) J. viviparus, or J. Conradi. I made nothing else out except that J. polycephalus Herb. Torr. & H. is J. ~~scirpoi~~ paradoxus, Mey. – and J. echinatus & nodosus is the true J. polycephalus Michx – but previously named J. scirpoides by Lamk. – Wednesday. After a careful exam. of my specimens of the Cambridge Juncus above referd to – & others in Herb. Greene (consid. by him a doubtful pl.) I think I will venture to say it is J. lamprocarpus, Ehrh.! in wch opin. Mr. G. is also quite disposed to agree. As suggestive of thought I will here refer to Hooker's Fl. Scot. where J. polycephalus! is enum. among the pl. of Scotland – which plant is in the Brit. Fl. referd to J. lamprocarpus!! This seems to me very valuable as shewing how much our plant seems in the eyes of foreign botanists like theirs – just as J. pelocarpus Mey was ref. to one Europ.n species (acutiflorus) by Hook., and to another (J. lamprocarpus) by Sprengl. – In brief let me now arrange our group as my present knowledge shall enable me.

1. J. nodosus, L. Sp. Pl. 466., Torr. Fl. 361. – J. scirpoides, Lam. Encycl. fide Mey., Mey in Linnæa., Kth. Enum. 3. 341., J. polycephalus, Michx. (fide descr., Kunth Syn. sub J. scirpoide, & ISC. Greene Obs in Herb. Torr.) Mey. ~~Herb~~ June., Curtis herb. (fide specim.) J. echinatus, Willd. herb. (fide Kth.) Muhl. Gram. (fide Mey.) Bott. Amer. pl. (canon omn.?) J. Rostkovii, Mey. June. (fide Torr., descr., & ic.) J. macrostemon, Gay (fide Kth.

Hab. Amer. septent., & merid., ~~Boustaalien~~ fide Mey. Differs from the European Juncei as indicated in the observation of L. in loc., – and might fr. this and the descr. alone indepd.t of the synonymy wch I cannot consult – have been safely pronounced to be L's plant as it was by Torrey & by Rostkof. From the latter's figure Meyer (asserting that many sp. are incl. under the Linnean name) gives it up and) makes out his J. Rostkovii. – In like manner Michx. makes out his polycephalus (seeing as it did certt. diffr fr. the European sp., & the Linnean pl. being taken to be doubtful) and cites the very same figure of Rostkof. The other synonyms are probably correct. – Our plant is very like in minutiæ characters to the following but seems always distinguishable by its few dense globose capitules more or less echinatish owing to the acuminate sepals as well as the acuminate capsules.

2. J. paradoxus, Mey. Junc. p. 30., Lahary. Junc., (fide Kth.) Kunth Enum., E.F. Enum. pl. Cantabr. ined., – J. polycephalos, Pursh (fide herb.!) Ell. Carol. (fide Mey.) Torr. herb.! & Flora., Gray!, Oakes! Greene! Beck! & auct. pl. non Michx., nec Mey. J. subverticillatus, Darlingt.! Cestr. non Willd.

Hab. Amer. Septentrionalis. When the whole history of this plant shall be understood – it may be found to belong to the last species – but is conveniently kept distinct – and is distinguished from the last by its smaller heads and supradecompound panicle – and from the allied foreign species by its acuminate sepals &c – as see the last sp.

3. J. lamprocarpus, Ehrh. cit. auctt. Kunth Enum. E.F. Distinguishable by its Hab. Near Boston; Herb. Greene. Cambridge; E.F. smallish dense chocolate-col.d heads – and differing from the last in its sepals which are not acuminate – the outer ones mucronate & the inner obtuse; H. Meyer refers to Sprengel (Neu Entd.) as having pronounced J. lamprocarpus an American plant, but says quite positively that he has never seen "genuinum J. lamprocarpi specimen Americanum". I am willing to take the risk of saying that my Cambridge plants and those of Herb. Greene belong to J. lamprocarpus, whether or no.

4. J. pelocarpus, Mey. Linz. Add. p. 30., & in Herb. Hook!, Kunth. Enum. J. acutiflorus, Hook! Bor. Amer. non Mey. fide Mey. – J. lamprocarpus, Sprengl. l.c. fide Mey. non Mey fide ips. nec Auctt.

Hab. Amer. Arctica, Hooker! Burlington, Vt. E.F.! Niagara, Herb. Greene! Differs very much in habit fr. J. acutiflorus and in its sepals as they are descr. by Meyer & Kunth. From J. lamprocarpus it is pronounced by Meyer & seems distinct – but belongs apparently to this species rather than any other, if not found to be distinct.

~~5. J. examinatus Michx~~

5. J. pallescens, Lam. Encycl. (fide Mey. e specim. Lam.) Mey. June. – J. acuminatus, Michx. Fl. (fide J. S.C. Greene! in Herb. Torr.) Torr. Fl., & Bott. Amer. Hab. Amer. Septent. Michx – Amer. merid. (Buenos Ayres) Lam. Meyer very positively refers our distinct & well known Michauxian plant to Lamarks species on his specimens – but Lahaspe, says Kth., does this with an erotesis – and therefore Kth retains the name of Mp. It is, at least commonly, remarkable for its long capsules calycem fere duplo superantia; as well as by the strictish habit of the panthela.

Here then I have ventured to do what I at first thought I could not attempt. I have no doubt at all about my specimens – & so far as they go – think JJ. lamprocarpus & pelocarpus well-marked; but 50 more of each are necessary at least. This will not however be regarded worthless by you; for the first 2 and the last are no doubt right – while the 4th is J. pelocarpus fide Mey himself – and the 3d I will be rash enough to "pronounce" identical with the foreign J. lamprocarpus. But there are 2 species to be added to the above – and then I believe this view of the northern species allied to nodosus will be complete.

6. J. militaris, Bigel. Fl. Bost. & Auct.

Hab. Mass., New Jersey. Greene! Gray! H. Very distinct in its characters and habit; & not requiring particular study here.

7. J. Conradi. E.F. mss. culmo erecto foliato foliis erectis compressis loeuloso-fistulosis, ~~&~~ anthela terminali subdecomposita floribus solitariis sepalis scariosis acutis capsula oblongo-ovata acuminato-rostrata brevioribus. – J. viviparus, Greene mss.!, J. viviparus, Conrad in Trans. Acad. Phil.! (fide Herb. Torr.) J. dichotomus, Torr. herb.!, Gray, Oakes &c. ex specim. non Ell.

Hab. Massachusetts – Greene, Oakes, Tuck.n – N.Y. Torr – N. Jers. Gr. – South Carolina, Greene!

Elliott's plant with flat leaves & globose fruit can hardly be otherwise than related to J. tenuis – as ref. by Mey. & Kth. If wood Jean to be the Southern J. tenuis – and specimens answering well fr. South Carolina are in Mr Greene's Herb.m. – This finishes our Juncei nodosi. –

I hope I am not too positive in the above, & that you will treat it with consideration. Having the Linnæa I have been going thro' it systematically — & had the luck to encounter some American pl. pub'd by Schrader in the Index Hort Goett. 1831 which do not seem to be mentioned in any author at hand. There are 3 species of Arabis, from arctic Amer. and a Trifolium Missouriense. If it is the Linnæa for 1833. NO! If they be valuable to you I shall be much pleased. It was quite unexpected. That is I was not looking for them In one of Chamisso's papers I find he recognizes Alnus viridis in Kamschatka — & this with Spach's judgment (if I remember right) & other reasons leads me to doubt if our A. crispa is distinct. The 2 sp. cent' look alike. I have asked Oakes to examine & settle it for his Wh. Mtn. Flora. Then I find Cham. admits Salix myrtilloides as a Boreal Amer. pl — some little confirmation of my ref. of S. pedicellaris to that sp. in Sill. Jour. He also describes 2 or 3 arctic willows without being willing to name them — & one of them I think must be S. Cutleri.

Many thanks for your precious gift of the famous Bauhins — & yet not last for Darlington and Baldwin. They are invaluable — & the former I should not have expected ever to have. You are resolved that I shall be in your debt: for my two pictures were of little account. I have two heads of Vahl — two of Link — and two of Dr. Porter (I dont know why he sent me 2 — nor why the worthy man ever submitted to the process) I shall be delighted if you find it agreable to take any of these. There is a head of Waterhouse, the first author on Botany belonging to this Flora of the present cent' — indeed the first after Cutler who wrote in 1785. You have looked at his "Botanist" I suppose. I bought the print in London. My Cambridge "Enum." has been finished since last winter — & is in Oakes's hands — who said at first that I had nothing to do with Essex — but finally agreed to fix the Essex plants. I will bring it to you when I get it. Will not it after all be best to go back to your plan & make it a useful manual. The fact was I had an ambition (have had it in view for years — & begun my Catal in 1839.) to be enum. among those who have illustrated the Boston Flora. There are one or 2 new notions carried out in the Catal. I always quote the orig' descr. for species. I will not say Desmodium viridiflorum, "Beck" — but it is "Desmodium viridiflorum (L.) Beck. So in my List of Lichens I have called the Lecidea sorediata of Muhl — a Parmelia — & the modern way would be to put my name alone after — & take the whole credit — but how much better is it "Parm. sorediata (Muhl.) Tuckerman" So far as I know this is original — & the best plan I have seen. No Botanist can make any mistake without feeling that he ought to have known. Again I mention the year in wh every plant became scientifically known by book. Then Josselyn gives a good list "Of such plants as have sprung up since the English planted and kept cattle in New England" which gives us the dates of most of such plants as Dandelion Shepherd's Purse &c — and these are determinable in all cases by comparison of Parkinson & Smith. Then Josselyn has another List "Of such plants as are proper to the country and have no name" wherein he gives figures & descr. of Symplocarpus! Gerardia! Mulgedium? Nabalus? Chelone! Trillium! Sarracenia! In his other Book he will describe Oenothera biennis though under the name of Lysimachia wch Ray & Morison called it. Then Cutler a century later 1785 — made known 381 sp. the Ferns Grapes & Sedges being excluded: for all wch he is cited. Again Cutler (like Josselyn.) says Symplocarpus "must undoubtedly be considered a new genus". His pub work is not half such a monument to his memory as the unpub MS. Flora of wch I have seen 3 "books" would be. There Comandra & Microrhynchus are scientifically erected into Genera & named. This was in '89. A little work with a sketch of his life extracts from his MSS — to shew what he found out all alone in Ipswich & what he knew with a portrait I shd delight to get together & mean to try to hunt up his other MSS. He was M.D., D.D., LL.D., I believe — a member of Congress — the great Philosopher of the whole country round & minister of Ipswich & Doctor too. On his death-bed he regretted that he had let his time be engrossed so much by Botany! Excuse all errors in the above from yours ms sincerely & resp'

Edw. Tuckerman

P.S. I have a parcel all ready for Dr. Torrey. Will there be any way of sending it at present? — May I trouble you yet further: ~~I~~ I promised a great many botanists in Europe to send them some plants, and those where I recieved spec^ns, I always gave all I could — yet almost always the balance was against me. By sending once I think I can finish a good many accounts — which I shall never get off my mind till I do: and I am willing they should be in my debt. When I think how kindly the great Martius, the admirable Zuccarini & recieved me — who could bring so little of interest to them — I feel I must satisfy all their expectations. To Sir Wm. Hooker I must be — want to be, always — in debt to an extent which I cannot discharge. But parcels for him will be a great while making — for I mean (if possible) that he shall smile when he turns them over. I have got at last to feel the extreme importance of complete sets of capital specimens — ~~with~~ such ~~I~~ will be welcome even at West Park, where common poor Ames, plants are almost coals to Newcastle. What a delight it is to look at the specimens of Hoppe, (too much prized however) Booth, Oakes, Sartwell. Such I have now the ambition of making. I mean however not only that the specimens shall be clean handsome pinched ones — like some Maples of Oakes's

wch are handsomer than any I have ever seen of Nature's exhibiting — though this is very well — but that the plants shall shew their metamorphoses — their colours their varieties &c. Some of the commonest plants will shine lustrously with this treatment. In this way I am confident of being able to be useful in some degree to Sir Wm. H. — & look forward & have done so with the greatest pleasure — to that parcel — wch shall be made up if I am able to ramble again in woods and fields. My design however is only to satisfy the Europ^n Botanists who gave me plants — and a parcel added to one of yours would do this. To send my Lichens & everything else whatever to Fries, for this great man has already grasped the whole System of Vegetable Nature — and everything comes to him eloquent as type or deviation — and I think I shall have no competitor in contributing to his stores — where everything will tell — my Lichens also to Borrer and Greville — the same to Hooker with all else that I can make worthy — & the same to Dr. Klotzsch — who is another that I shall have to send to all my life — but I am frightened already: & cant think of Maire of Paris, to whom I owe a good set of Carices — & Montagne Lichens. I say if I send the above — & continue to send Fries & Hooker & Klotzsch it will be all I can do. I shall expect nothing back of course except fr. Fries. I promised to get him Schweinitz's Syn. Fung. and the Specimen Museorum. Perhaps the Amer. Phil. Soc. would furnish a copy of the former. Or could 2 be bought?

But I did not mean to trouble you with all this. I only wished to ask you if you could not conveniently on some occasion let me add a parcel for Prof Zuccarini one for Dr. Hoppe of Carices & perhaps another, to yours, when you send again. If inconvenient it is no matter — but I would rather not send them separately for I cannot continue to send: and these are only to shew them I am not insensible to their Kindness when I visited them abroad. I am ashamed to send you so much talk; but so it seems to be.

Boston 8 April 1844.

My dear Friend

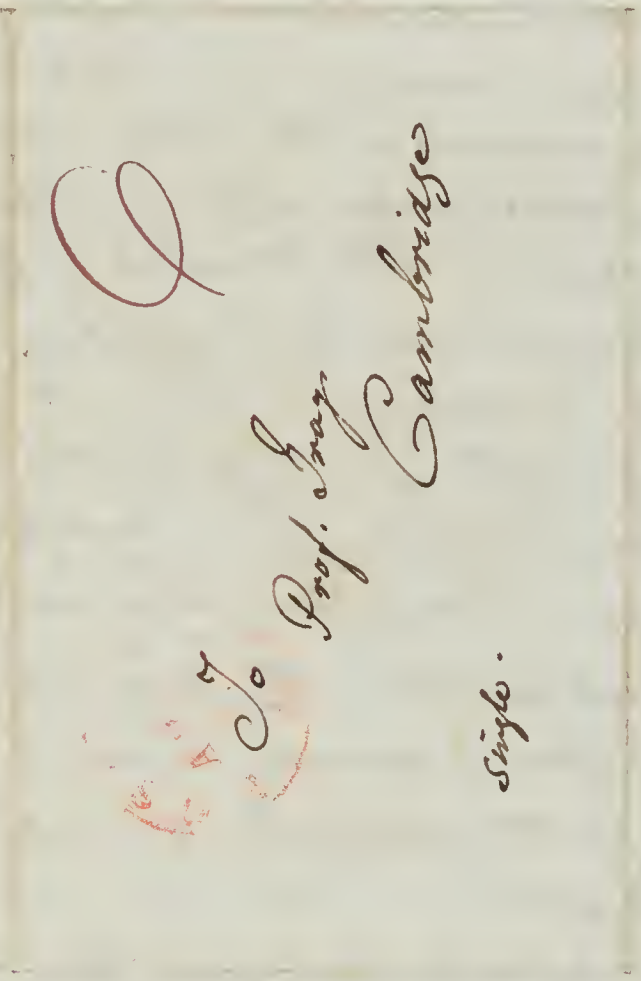
Your valued letter brought me a happy morning. Many many thanks for all its kind suggestions. I will avail myself of your permission to attend your lectures. I was very glad to get the Month as I had only as far as the 3d. For the first time does an American name appear in the running title of the pages! Further than this we cannot go — but may this not stand alone — may others gain the same position! We depend no longer on Europe; only as Europe depends upon us, in mutual ties. As regards Hooker, being wholly unacquainted with the work, & a little wondering how an English work, & by the magnificent Sir Wm H. himself, should be offered at $3 — I would say, that if you have no other way of getting rid of it — & I can be sure of the Completing part, I will take it. We have so few ferns — and hardly any fern-houses, that a work of this kind can hardly be practically useful here.

Dr. Knieskern has given me an opportunity to communicate a little piece of news—
He sends me an unnamed Carex from "banks of streams, N. J."— which is C. salina I am sure.
Greene is thus confirmed. So much doubt attached to his discovery — I mean from his own uncertainty where or when he got it — & his general doubts—
Hooker I was sure in regard to his specimen in Herb. as Dr. B. was before — but my acquaintance with the living pl. gave me a right to an opin.
Booth— That Dr Knieskern's discovery is very valuable. I hope next summer to find it here too.
The plant does not seem in any respect diff[t] from the Swedish. Dr. K. reads only 3 spec[s] — but they are sufficient to form an opinion upon.
Nothing else new from him except a splendid set of C. Barrattii, C. livida &c.
I wish C. Barrattii was as like C. glauca, as C. livida is like the foreign plant. That I cant think distinct — but then it belongs to a much clearer group than C. Barrattii which must probably, after all (as already pronounced by D. Booth) be given up.
Dr. K sends CC. bullata & cylindrica of my pamphlet as distinct — both looking like my plants.
He sends capital C. exilis with 1—2 additional smaller spikelets — the tendency to which development was noticed in my pamphlet — though I had not for the state well marked). My locality for this is Lincoln on the Concord Turnpike, a few miles this side of Concord, where it grows with C. paniculata & Eriophr. alpinum you will with Lobelia perhaps. Near here Habenaria hyperborea was also found.
As an old member of the Boston Society I feel delighted when I can do anything — I was adm. in 1833 — and used to attend pretty regularly — But they are all Zoologists — except Mr Teschemacher who is indeed a Botanist — but frankly says he likes Mineralogy far better. So what with his measurements of chrystals — Mr Alger's account of some stones picked up in Chili & Dr. Wyman's investigations of bones — & Dr. Gould's of mollusks — there is little for me. Besides I am not fond of these public meetings — I dont think true working naturalists ever are, as fond as others. But we may well as New Englanders be proud of our Society. Such men as form the Council of the Boston Society would honor any Academy in Europe.
But how I run on. I will send you the money for the Books, & hope to visit Cambridge in the course of a month. I have not written Dr. Booth, who has not yet replied to my last letter — but I mean to put up a parcel for him directly, when I will write.

I remain my. resp[y] & sincerely yrs

Edw. Tuckerman

P.S. Excuse me that I have not sent this before. But my health has been worse & worse — & now I am going north to see whether it will improve; hoping to take Ipswich in the way. Indeed if all is propitious, and I can get a chance to run about there for a week, I shall stay. After this I hope to come out minded to Cambridge to fulfil an inv. fr. Dr. Harris when I am sure I shall get a chance to see you — & if you so please to enter upon anything there is, or you may suggest.



W. Boott Esq.

Lowell.

Dear Sir

I rec'd the enclosed from in a parcel from your brother, my kind friend Dr. Boott. I was doubting how I could send it, when I thought of the Aspid. aculeatum, which I was so fortunate as to discover on the White Mountains last fall — & which I had long intended sending you. Please accept these specimens — gathered in October — & excuse the haste with which I am compelled to put them up — being just about to leave home for the summer.

With respect
I am yours truly
Edw. Tuckerman

Boston
27 July 1844.

Cambridge 8 Nov. 1845.

Dr Gray

Dear Sir

I regret extremely that any words of mine should have given you such just occasion of annoyance. I did not indeed use the words as stated — but I intimated certainly that I didnt know that I had had all the messages that were sent. I have told you, and you know my weakness in this respect — which can alone account for the state of feeling which made such words possible.

That I regret them in nothing. I am most truly sorry for them — and ask you for-giveness — assuring you that you shall have no other cause of offence from me.

But I have told you all & more.

Believe me you shall never have me for such wilderness again Ms. resp'y yr obliged frnd
& servt Edward Tuckerman

To Dr. Jean
Cambridge

Dear Sir

I have just put up the Washington Lichens & shall place them in the hands of the American Minister tomorrow. I have also written to Capt. Fitches - informing him of my being disappointed in reaching London in September owing to the severity of the Quarantine on all persons arriving in Sweden from Hamburg - & my subsequent proceedings in regard to the plants.

I put them into D.r Montagne's hands - not being able myself either to leave our party - or take them with me to Paris - through a Messenger - & need his reply sometime after - & a second note which I find necessary

despatch to Washington tomorrow. The Manuscript is very plain - & there is so little of it, that I feel confident it will not give you much trouble. I should greatly desire to have the quotations & authors of species printed according to my own way of doing it - if this be within possibility - Indeed I do not see why I should not have the right (except ex excipiendis) to adopt what appears to me the proper course with my own article in this respect - provided always, as above that the Expense be not too great. Your views I believe are in accordance with the general usage - & this is the reason I venture to renew here my dissent from the received plan, that it may have your consideration, when the affair comes up.

It is a trifle in the general question but hardly so to me.

I have only room to ask you to be kind enough to command me, here till March, at least, & in June in London if I can be of any service, & to subscribe myself

Yours truly E. W. Teschemacher

Lisbon 12 Dec. 1849.

P.S. Pray excuse my writing on this page - but as it is the last I have it all to myself - & will not trouble you with another. I have left a space for you to fill up with the number of species in each genus - & the name of the author of the species - as I have not the means of doing it myself. But I do not wish you to undertake this unless you feel quite at your leisure & disposed to do so. Should you not feel inclined to undertake it, I will endeavour to find some one who will do it. But I do not like to trust the Manuscript out of my own hands.

to write him I only waited the answer to, in order to get the work done as above-mentioned. The order of Capt Wilkes that the Specimens shd be returned in Nov^r would have been answered by their return in Oct^r if I had only been able to get to Sweden. I have no doubt, that in your view at least, the increased value the List has gained will be a quite sufficient element for the delay. I only wish I had been a little more conversant with the Business of preparing Plants for Engravg directing the Engraver &c — for I know nothing about this part of the work nor how it has been done.

When I get home I shall hope to put a fine microscope by Schick into use & then follow out to the best of my ability the kind suggestions you so freely offer as to the importance of these latter investigations.

All were well in England that I saw of your family. I even saw Sir Wm. Hooker looking as well. Dr. Booth the same. Mr Brown inquired particularly after you as did Mr Bennett & Sir Wm Hooker.

I have not seen many Botanists in Germany as yet. Dr Klotzsch, Prof. Link & Prof. Kunth are well — as also Profs. C. Schlechtendal & Lehmann. The only news is the lamented death of the venerable Koch — 70 years old.

But I am forgetting to mention the main thing I had in view in writing you (when I made so little to communicate) I that is — to commend to you kindly the two sheets on Lichens that I shall

Amherst 28 Dec. 1854

My dear Friend I was suddenly called to Boston some three weeks since by the dangerous illness of my wife's father, my father & old partner & friend, Mr Cushing. His death has kept me in Boston till today, when I reached Amherst & found your welcome notes of 1st & 20th instant for which accept my thanks.

If you will send the box by Buck or Sawin to Mrs. Cushing's, No. 10 Mt Vernon Street I shall be able to settle charges & pay myself in some ten days.

I got a little note from Fiske the other day — but was much relieved to be disappointed

In your absence I went down, but indeed I spent only half an hour in Cambridge — except on Sunday afternoon when I went out to extend my friendly to attend my Father Cushing's funeral.

I beg to present my best regards to Mrs Gray —

& to Asa

Yours sincerely

Edw Tuckerman

Jr

J. Gray.

Cambridge

that he said to Leiblein about a remarkably fine collection of things which I sent to him, — & especially about certain cruces among them on which I desired his opinion. Now I hope for more notes as well as specimens.

I am a Lecturer on History, here, & am busy on my lectures. But I have two new volumes (4 fasciculi) of the Lich. Exsiccati now in the Binder's hands in Boston — besides, — & there I expect to fill a despatch before Summer. I have neglected it if I do not anticipate, to send you the work — but will do so, when the two new volumes are ready. If Fries has considered & answered my queries [I fear he has not, as he talks in his letter as though he had given up Lichenography to his son] I should certainly endeavour to bring out either a new work on N. A. Lichens or at least a Supplement or other Separate work on Southern Lichens — for which I am nearly ready — before the coming Mid-Summer.

But my lectures must be delivered before I shall feel quite ready for the last mentioned labour.

Enough to show you that I do not think of deserting my studies in Botany. This is a very pretty place & has some collections in mineralogy geology & zoology — & a most genial natural philosopher in Dr Hitchcock whom everybody knows.

I wish I had called to

Amherst 20 Oct. 1855.

Dear Doctor

Let me thank you for the valuable memoir, which have reached me — and also congratulate you on your return.

I hope to do some work on Lichenology this winter.

May I also congratulate you as a Member of the Academy of Berlin. America has received few such honours.

I mean David Lee as soon as I get time to take up the Lichens, to which I have appro-

friend the present writer.

I sent him a Box in 1853, containing, besides valuable American Books & plants – some Southern Lichens upon which I wished exceedingly his judgment. I do not get know in any way, whether he received the Box, as he has not acknowledged the Books, nor noticed the Lichens. In his last, accompanying the Parcel which con[illegible] sent me last summer, there is a general acknowledgment only, by which I cannot tell whether he has got my last Box or not. It was sent through the Swedish consul.

My new publications on Lichenography have existed now five years in consequence of the above-mentioned failure. It seems hardly possible that the box can have reached Fries and not for Lichens.

I remain truly yours

John Tuckerman

To
E. Fay
Cambridge

Amherst C Nov. 1855

My dear Friend

I have not have delayed replying to your kind note of the 18th ult., but for the engrossing labors of Farming. I bought a small place on the verge of the village last spring, which had some fine apple-trees. It is a good apple year in this part of the State, and I find myself overwhelmed. When I got back under cover 200 barrels of apples. Besides this I am getting the place in order by draining, & to build

next Spring — and the time is short. Next year I shall be glad to send you some [illegible] apples if you want any. I had about 70 Bbls of Rhode Island Greenings, and there is scarcely any [illegible] fruit on the place. Indeed this is a very fine region for the finest of fruits, and the display at our Cattle Show astonished me.

As I suppose that the Report will, I send the little pamphlet for Dr Darlington & Bost & you. Shall write

not trouble you to despatch it to Mr Ravenel for him, & shall be glad to pay any expense of carriage.

I will write Bris, the first spare time I get. I write to him Latine, and as I do not know other occasion of expressing myself in that tongue it takes [illegible] to do it.

I hope to do all I can this winter in Lichens. The books for vols II, & III, & Exsiccati, are printed & bound, & await only the specimens

Friendly yours
Edw. Tuckerman

Amherst 21 March
1856

My dear Friend

It gave me great pleasure to receive the news of Venezuela lichens collected by Mr. Fendler, and placed at my disposal by you. I shall be glad to get them at such price as you decide on. I have nothing at all from that particular region. I wish I knew how to tell Mr. Wright that I still hope to be remembered by him. Perhaps you may be able to say so — and add my hearty good wishes for his health. It is very long since I heard how or where he was. I have been at work on Lichens, now a month. The Mechanical work of getting out the Exsiccata is very considerable, & I could not have done it, had I not declined in season to write a new course of lectures for my Class,

for the copy Sumner. I wrote to Mr. Peters, making a general Survey of everything I could find from him, & enclosing a considerable no of specimens which he desired, & hope the letter reached him.

While I was absent last fall Lord, the Earl, came a letter from Mr. Chas. Jared Sprague (I am not sure if it be the same gentleman who was long your Secretary) desiring a list of Works on Architecture for the Committee on the Selection of Books for the Boston Library.

As I am situated here, and shall be, till my house is built, it was impossible for me to give him a complete one, & I therefore postponed writing for some time. At length I sent him a list, which I regarded the most important books, so far as I could refer to them. From his reply I find that I unwittingly gave you the trouble of making out a list — which I much regret, but do not see how I could have helped. Nobody is more sensible than I of the importance of several works in the Appendix to Cyclopædia, Cuba, & that to Webb's Canaan, & other works I do not possess — & wish these costly publications might be made more accessible.

I hope to have the pleasure of bringing you my Excavata, when I come to Cambridge in May. If it be possible I shall do it.

Believe me Sincerely Yours.

Wm. Strickland.

Amherst, 27th March, 1856.

Dear Gray

I hasten to answer your letter just received, being especially awakened by your allusion to Potamogeton. I have materials for a continuation of my monography, including descriptions, & divs, like the others published, of quite a number of species, two of which are I suppose new. And I have so much confidence in the part of the work already done, that I much wish you could find it possible to examine my own specimens shortly, before you publish the revision in the new edition of the Manual (a book I long for) I will send the collection to you, and also my exotick collection, at once if you will wait for it. Fries has my copy of the Manual, in the first edition, and I have not yet had time to get another, so that I cannot tell how far you there concurred in my conclusions — but I feel sure that P. Claytonii, P. pulcher, P. amplifolius, P. Niagarensis, and the rest will speak for themselves in my large suite of specimens from various parts. P. amplifolius is as distinct from P. pulcher, as any other two species of the section are so far as I can see — and I have waded and boated a good deal into their peculiarities.

There is one new species for which I wish to solicit your regard, and the more so that I owe the designation of it to one of the earliest and best of botanical friends, Dr. Robbins of Upbridge. It is the P. trichoides of our botanists. I satisfied myself completely when last in Europe that our plant was remote from that of Chamisso & Schechtendal (I have indeed an original specimen of their plant) but had not provided any other name, when Dr Robbins sent me some of the exquisite specimens that he makes of the more delicate Potamogetons, with the name P. Tuckermani, attached, which I promised to accept and adopt. Besides, I have a single specimen (quaerendum valeat!) of Potamogeton marinus, collected by Dahl in Greenland, which is the only authority I knew of for putting that species down in our Flora. The "overwhelmed Potamogeton" (it required genius of a certain sort to conceive of that name!) of Mr. Wood, I knew to be P. vaseseens, but have no specimen from the author. It is difficult for me to guess at the reason which induce Mr. Jay to separate P. heterophyllus and P. spirillus from Potamogeton. But it is agreeable that he finds my new species a probable one, if indeed he does.

The lichens will be most acceptable at once, and I suppose will come safe by Express. I have now been steadily engaged since the 1st Monday of this month in preparing the 2d volume, or 3d & 4th Fasciculi, of Exsiccati, and have done nothing else, and get it only half through. Indeed the work requires the careful selection, and affixing to the page of no less than about 7500 specimens of lichens; so that I fear only one volume may be got through before other duties interrupt — instead of two, as I expected. Both the books and the specimens are however all ready for the 5th & 6th Fasciculi, and they will be finished as soon after as is possible.

I often think of my poor labours in behalf of the Exploring Expedition collection, and with some solicitude. Twice I have done the best I could for the illustration of the collection, and still years have passed since my last revision, and I feel as though I could do better yet. The French botanists who may look at the memoir will certainly find fault with the meagreness of the figures, the want of anatomical details — and especially of reference to the figures in the descriptions. I fear I am not by nature a microscopist, but I have

a strong desire to do all in my power that my part of the work may not bring discredit on the rest. Let me then say that if any further labours upon the memoir in question will be acceptable, and are considered by you desirable, I will gladly undertake them, and revise my whole memoir without charge to the Government; provided they lend me the mss. and the specimens of new species contained in it.

I have thus, encouraged by your friendliness told you all my story, and I am sure excuse me — and to add only that I am

Yours sincerely

Edw. Tuckerman

To
Dr. Gray
Cambridge.

Amherst 2 April 1836.

Dear Gray,

Your letter gave me much to think of. I heartily wish I had conferred with you abt Potamogeton. I enclose however the ripe fruit of P. gmelini, P. amplifolius, & P. lucens — the last two from Fish Pond, & I add the leaves of the last to leave no room for doubt. These fruits are what I described from & they are certainly marked by considerable differences: beside those of the leaves. Supposing however that P. amplifolius prove to be a form of P. lucens (which I cannot think it will, as I now know it) — it seems to me that P. gmelini is a distinct & separate thing. I have it from St. Louis (Riehl.!) with the same char[s] as to floating leaves, submerged leaves, & ripe fruit as it shows here. I also add P. Claytonii a truly peculiar American species — and P. Tuckii of Robbins.

Nolte & Vieil print it the true plant in 1849!

to hand, but I hope for them tonight. Please to keep the specy sent. I wish I could despatch the whole.

Yours truly

W. J. Hooker.

My full & elaborated descriptions of all these new species – including P. Niagarensis (of which I send the ripe nuts) are in Silliman's Journal for 1848-9.

I have tried in vain to find my copy, & I would send you the descriptions – but I hope you will look at them — They embody a great deal of honest labour, and cost me some chills in the Lakes of West N. Y. and I am sure you can do them the justice they ought to have.

My specimens are very full & fair of all the new species — & I will send you the whole if you say so.

I saw "Pot. crispus, Delaware, Griffiths," in Nolte's coll^n of Potamogetons, which he said, & which I saw, was the true plant.

I saw P. Fuckemani from the Alleghany entry in herb. Nolte. He consid^d it distinct from P. trichoides, & was unable to refer it to any other publ^d species.

P. gracilis of Fries is a salt-water species, which he says (Summa p. 217) he never gath^d but once, and considers most likely to be confounded with the small var^t of pusillus, from which he says it differs, besides fruit, in having the exact habit & vegetation of P. trichoides — This habit & vegetation of P. trichoides appear to me quite different from those of P. Fuckemani. I send some bits of Nolte's specimen of P. trichoides, as this a rare species in European herbaria, & possibly also with you. These spec^s are quite the same as others which I have from Kunth, Reichenbach, & Schlechtendal, and there can be no doubt I think that P. gracilis & P. trichoides are nearly related cousins of P. pusillus, while our new American plant which Robbins has named is much more distantly connected with the latter species.

The Lichens have not yet come

that name, as I was unable to consult it, having no specimens. I very soon discovered the error however when in Europe, & distributed the specimens there under Dr Rothrock's name.

As to P. flaccidus I supposed that I had it from Wm. Newyork — and have no reason now to think differently. I have not examined it with a view to what you suggest of its possibly including other species.

I hope to go on with my examination of the genus as I have opportunity. There is a curious species about Boston (P. spathis, J. sp.) which was considered n. sp. by Stolle, as I had myself before regarded it, & what I have recently been able to see respecting — and another in the Connecticut on the same category. There is also a curious Juncichellis-like thing in Medford, and also in Hybrid which is with little doubt new, but the fruit is as yet unknown. When you publish again, I hope all these things will receive a full and satisfactory examination, as well as the new species which I have already partitioned. P. Claytoni belongs to the group which includes P. compressus — with truly linear leaves — and is the most remarkable species, as it seems to me, in the group. It has certainly no direct relationship to P. graminifolius, if I know it rightly.

I did not mean to ask you for the specimens sent — I shall hope to supply deficiencies in this genus — when I know exactly what you require. But I would say that it is rather difficult to obtain respectable specimens of P. lucens (or I have been so with mine) and the one I sent is therefore worth preserving. P. amplifolius and P. pulchellus are very good about their fruit,

Amherst
15 April 1856

Dear Gray

I cannot help feeling sorry that my paper on the Podomyctera escaped you, and I certainly ought to have sent you a copy — but as I remember the copies came just as I was preparing to go aboard, and I availed myself of them to put one into each of the parcels of species which I made up for foreign friends. In this way they all went; for I have none myself, except that contained in my copy of the Journal, and that I have not been able to discover since I got to Amherst; and probably shall not now till we get into our new house.

I am very glad I sent you the Specimens, as D. Tuckermanii will have its rights as a very common species. I found this at the White Mountains, & took up the name of Nichoides for it, & you will find it described by me under

Just P. Lucens, though it grows abundantly and finely in Fresh Pond (in rather deep water off the high bank at which the Hotel stands, where the bank subsides into the low shore) barely perfects its nuts.

I am glad Mr Tuckerman was content with what I sent. I feel poor this season, expecting to build, but shall be glad to be able to do more in future. As soon as the Explicata are done — I go to work on a new book to include all the North-American Lichens so far as known (a kind of Prodromus to a Lichenographia Amer. Sept. — and any thing to which you may be able to help me will be most acceptable. I remain Dear Gray

Yours truly

W. Oakes

that name, as I was unable to conceived it, having no specimens. I very soon discovered the error however when in Europe, & distributed the specimens there under Dr Rothrock's name.

As to P. plicatum I supposed that I had it from West^n New York — and have no reason now to think differently. I have not examined it with a view to what you suggest of its possibly including other species.

I hope to go on with my examination of the genus as I have opportunity. There is a curious species about Boston (P. spathis, T. mss.) which was considered n. sp. by Stolle, as I had myself before regarded it, & what I have recently been able to put together — and another in the Connecticut valley in the Jesup collection. There is also a curious Jeanneretellia-like thing in Medford, and also in Hyde's which is with little doubt new, but the fruit is as yet unknown. When you publish again, I hope all these things will receive a full and satisfactory examination, as well as the new species which I have already published. P. Claytoni belongs to the group which includes P. compressum — with truly linear leaves — and is the most remarkable species, as it seems to me, in the group. It has certainly no direct relationship to P. graminifolium, if I have it rightly.

I did not mean to ask you for the specimens sent — I shall hope to supply P. canadense in this genus — when I know exactly what you require. Perh. I would say that it is rather difficult to obtain ripe fruits of P. lucens (or I have been unlucky) and the one I sent is therefore worth preserving. P. amplifolium and P. pubescens are very good about their fruit,

Amherst 27 May 1836

Dear Ray

I thank you very much for your kind help in despatching my books — but I really feel as if I had been too eager for your aid. The fact is I was exceedingly hurried at the time I called upon you — the time for my lectures here having arrived — & I felt unwilling to delay — & every moment appeared important. I have sent the books to the Smithsonian Institute, and also an invoice of the contents of the case. I forgot to ask you if you had not had to pay something on a copy of the 1st volume of my Entomology which I sent to you last winter for Mr Van den Bosch. But I will leave this till I come to Cambridge, which will be I hope in a few weeks, as I have to study a day or two in the Library —

Yours truly

Dr. Peck

Amherst 2 July 1856.

My dear Doctor

I thank you for your kind favour in giving me the Preface. As I hope to get my knowledge into order in the course of the present year, I feel willing to undertake to supply the Lichens for your next proposed two volume edition — but there will be a great deal of microscopical work to be accomplished before the Systema Lichenum Amer. Sept. can take its place on Library Shelves. I mean to effect all I can towards that work — & not to lose time about it — but the longer your edition is delayed — the greater progress I shall be able to make.

The new school of Lichenists (whatever they may have effected with regard to the subdivisions of the System) have settled the question about microscopical characters so far at least as this — that these characters must be fully weighed in all our conclusions. I have not done

Jim's wrote me the other day. He is still doing nothing with Lichens, but his son Theodore is very forward to take his father's place — the latter proposes a Monography of Stereocaulon — He has already published a new volume of his father's Lichenes Succici Exsiccati — which is admirable for the character of the specimens. Still he is quite a young man, & it will be long before his father's disciples will be willing to give up attempting to set his opinions —

Believe me

Yours truly

Wm. Faulkner

much at present — no more indeed than I could help in the microscopical study — & I therefore see a great deal of it before me. That is all that will interfere with an early sketch of our Lichenography — & this I may be able to satisfy before you will require the MS.

Thank you very much for reading so kindly my books. You do not mention Mr. Brown's but I suppose you will retain that till you send your packet to which you refer.

I suppose it is far too late to speak of it, but I remember a slight error in the Synonymy of Carex in your last edition of the Manual — by which it was made to appear that Booth & Dewey had agreed to call the same species after me — while if I mistake not it was two different species which were then named. Perhaps this is too unimportant a criticism but it did not seem so to me, & I meant to speak of it to you, and possibly did so when the Manual appeared.

As to the Plates required for & due to the proposed new volume of your invaluable work — I shall trust you may be able to help me to find some suitable hand: or that at least some one may turn up in season. I am not in the way of finding one — I fear — & I should wish heartily that my part of a book with which you, Mr. Sullivant, & Mr. Harvey, are to be connected as authors — should fall, in all respects, it's place, as well as I can possibly make it. But there is no immediate hurry about that?

There is also a suggestion that occurs to me with respect to the proof. You have certainly expressed yourself in a way that I may hope either to verify in some degree to justify — & it will give me great pleasure to prepare a popular account of your discoveries — (and the more so if the popular character should permit me to give some vexata questiones, the go by.

Amherst 2 Dec. 1856.

Dear Docto

I wish to thank you very heartily for the parcel of Chilian & other Lichens, valuable not only on their own account, but several of them particularly so as being authentick specimens of species either ill, or not at all represented in my herbarium. Will you let me hope for more of the same sort?

The parcel from Sir Wm Hooker consisted entirely of Potamogetons, nearly all E. Indian & Australian species. Now I thought I put myself far in advance of others who had studied Potamogeton, inasmuch as I confined myself strictly to examination & description e vivo (my descr. were drawn up in every case from the living plant, where I gathered it) but if you adhere to your way of looking at them I do not see when we shall arrive at any good view of the genus — & so certainly may go on as before & make what they can of the shriveled bits

what to generally represent the genus in Herbaria. My specimen were never, I believe surpassed for excellence, & I do not see how the plant could be more thoroughly studied. Still, it is I admit, one of the highest questions which can arise in Botany, what characters shall be allowed sufficient to distinguish species, & here I defer to your judgment with entire satisfaction so far as I can feel certain that the Court has really had all the evidence properly presented before it.

But if (as I assert) the fruit of P. laceus is always distinct from that of P. serpentaria; & that of P. fimbriata from that of P. amplifolius, and these four are all distinct from each other why shd the genus Potamogeton come under a diff[t] category in this regard, from so many other genera (as Salix) the species of which depend on the fruit entirely for their separation?

I only throw this out because I am still doubtful whether you have seen as much as I have of these plants, & ask at all expecting you to take the trouble of replying. I begin to fear that you will think "Monsieur Potamogeton has come again!"

Truly yours
Edw. Tuckerman

P.S. I forgot to speak of the elegant book of Mr. Sullivant. It speaks well for the advance that American Botany has made. Would that I could get a collector to travel for me, like M. Lesueur, but I cannot think of the expense at present, at any rate, and there are no better correspondents than our Southern cryptogamical friends.

Amherst
1 Nov 1836.

Dear Doctor

I shall be very glad to receive the new Musci Exotici by Mr. Schwaegrichen. My collection contains now, besides his elegant Musci Alleghanienses (which has been aiding a worthy botanist, C.C. Frost, for some six months) the last 250 of Drummond's Amer. Mosses which I bought of Sir W. Hooker, and a very large set of British Mosses from Greville. I have not yet sent my 4th & 5th Fascicles of Lichens to Mr Sullivant, despairing the Mails, but shall do so, as the others thus sent have all reached their destination.

But I am sorry to say that not one of the numerous copies of my work sent to Europe has been acknowledged excepting by Mr Churchill Babington. And Montagne has not acknowledged the fine parcel of Venezuela and other Lichens which I despatched to Dr Henry for him. I confess I feel inclined to be vexed that I should have no news at all in regard to so many ventures. Can you give me any comfort? Is it that I mentioned generally in my letter that I hoped, within the year to send another volume, the gentlemen will wait and acknowledge both at once? But enough of that.

I have been perusing the new edition of the Manual

It strikes me as a less convenient & pleasant size than the first edition, and I cant help hoping you will go back to that when the work assumes the shape of two volumes. For so I fully sympathize as a northern botanist with the considerable slice of the Southern Flora (as we have hitherto been accustomed to consider it) which you have, doubtless on sufficient grounds, added to our northern territory. It seems strange to call the Live Oak & the Cypress northern trees. As respects the typography I have noticed scarcely anything to remark upon. But there is one point which I am going to ask you to set right. It is an universal custom in Flora to print the name of the botanist who has been fortunate enough to add anything of interest before unknown to the region, after his species. And with all your punctiliousness, you still follow this Engelz. Thus Baker is rightly mentioned as the original discoverer of Euphrasia officinalis as an indigenous plant of the U.S. Now what I wish to suggest is, that my name might with the same propriety be placed after Rhinanthus Crista-galli, since the position of that plant as an indigenous member of our Flora depends entirely on my discovery of it in Baker's field. Baker never thought of meddling with this. The discovery was as absolute as his of Euphrasia. Then take Subularia aquatica. Nuttall said he had found this in pools at Portland, Maine, but after Boott & I had searched in vain for it (as Wm Boott has done since) Nuttall gave it up as a mistake, and no New England botanist knew

anything about the grant. Well I found it in Echo Lake, Franconia, sufficiently abundant, but so local that M^r Boott tells me he failed entirely to get it — and supplied everybody. besides having seen the spot, — and this was my absolute discovery, and I claim the credit of it. I believe I mentioned this in my notes on the first edition — but in this the Maine locality which nobody has been able to authenticate, I alone (even to day) every American specimen now existing in any of our botanists herbaria — that is from the U.S., is based on, & the discoverer is* Enough of so small a matter, but perhaps you will think it worth setting right.
Just so Dr. Pickering denies the credit of Carex præcox, just as Gray of C. folia, Lanjata, & salina. Excuse these suggestions; indeed, if Lanuginata not, you invite them. Oakes takes pains, in his printed lists, to give Dr. Perkins credit for his discoveries, very unlike my when they were made, look at the White Mtns, Labradorica, and you do so in many other cases, as with Olney, Wood, C. J. Sprague &c.
As to the Potamogetons it is not possible that I should not be a little disappointed. I believe P. Claytonii as good a species as there is in your Manual; I know it belongs to a different group from P. heterophyllus, and that P. compressus might just as well be reduced to that species and to leave all the rest, as P. lonchites, which is doubtless near (not to P. natans however but) P. heterophyllus,

* It is not possible that you can suppose that M^r Oakes was the finder of Subularia, and Rhizanthus? I suspect he has put the contrary in print

of which I have often thought it seemed a vague form — let me say that I shall be glad if young botanists who use the Manual do not consider me as overjoyed and fond wanting in the matter of my studies in Potamogeton — while I put it to your sense of justice whether there has been any such adjudication of my rights.

As you were one of the editors of the Journal of Science I supposed that a paper of some twenty pages printed in the Journal would be sufficiently brought to your notice by that printing — and I do not see why such paper should not have been weighed, when the time came for it. But this part of the Manual is enough to discourage a botanist. To be sure my specimens are in all the principal collections, & will have their appreciative judgment yet, but why should they not have it from you also.

Potamogeton Spirillus is a more remarkable plant than "P. hybridus var. spicatus Engelm." It appears to me moreover more in accordance with science to distinguish as I have done two such well marked forms as P. pulcher & P. amplifolius — than to compel these, & three universally admitted European species beside, into one unmanageable & inconceivable "species."

Best I must stop. If you will kindly send the Report from Mr. Sullivant to Mr. Cushing, 10 Mt Vernon St, Boston to me, it will reach me. Will you let me, after such an impassioned defence of my Potamogetons, remind you of some Christian duties. They will be remembered to me. Truly yours

Asa Gray

P.S. I may say that I understand & sympathize with your views on the extension or new limitation of many old species — so as to bring many new ones under them. I refer to the observations in your last paper in Silliman's Journal, & to the scattered remarks in the same periodical in the Manual. I had myself wondered into the same train of speculation in a note to Jowelyn's Rariora of New England, an old book I have been preparing for a 2nd edition, without any knowledge that you were occupied similarly. But I had before this inclined the same way. In my "Enum° Pl. Agri Cantabrigiensis" 1843, a ms. ready for the printer ever since the date — I refer Rubus strigosus to R. Idaeus as "β strigosus, caule petiolis pedunculis calycesque aculeato-hispidissimis." In the same Enumeration, I refer Geranium Carolinianum, to "G. dissectum, β. Carolinianum pedunculis apice confertis." The Ribes rubrum of the White Mountains is made an American variety by the designation "R. rubrum, β. albinervium foliis abbreviatis, laciniis lobatis glabriusculis nervis exalbidis (R. albinervium, Michx.)" Chrysosplenium oppositifolium stands as "C. oppositifolium β, floribus minusculis foliis superne nunc alternis. In specim. acto Anglicis ex Hb. Boott folia variantia, nunc reniformia, nunc magis orbiculata plus-minusve crenato-dentata s. repanda. Flores in specim. tribus distantes solitarii sicut pl. Amer." I am still not certain that Schweinitz has rightly separated as a species (Michx.) Pyrola asarifolia, but this note, "Nil nisi varietas P. rotundifolia mihi" Under Vaccinium humosum, is "var. β. Pennsylvanicum, foliis fructuque minoribus (V. Pennsylvanicum, Ait., Fl. Bost.)" Frutalis is then stated, — "V. vulgaris, Michx. (V. Europæa, L., V. Americana, Pursh.)"

Salicum septentrionale, Fl. Bost is made "S. boreale, var. Americanum, caule glaberrimo nodis puberulis panicula longiuscula subpyramidali ramis erectiusculis paucifloris. Planta Britannica nunc omnino pubescens nunc glabriuscula polliceque plus minus ciliatis accurrit, vix forsan glabra. panicula pyramidali."

Rumex pallidus, Bigel. is referred to R. persicarioides, L. with the obs: — "R. pallidus noster a Rumicibus Europæis certè differt, bene attamen cum R. persicarioide, specie Americana Linnæana convenit. E descr. Bigelovii specieminibusque lacinia perigonii pl. rostrâ vix dentibus præditæ sunt. Character fortasse haud multum valet; nec lacinia in roriba semper integra." Salicornia mucronata, is set down as "S. herbacea, β, mucronata, mihi, articulorum dentibus squamisque conspicue mucronatis;" and our other species is thus distinguished: — "S. Virginica, L., Willd. Species diversa caule (ut videtur) subfruticoso, ramis plurimis gracilibus saepe indivisis spica longa terminatis: S. ambigua, Michx. hujus varietas humilis. Conf. et S. radicantem, Sm., Hook. Amer."

Chenopodium maritimum Torr. Fl. is thus arranged "Schobera linearis (Ell.) Mihi. Semine majori fusco lævissimo, foliisque majusculis longiusculis a S. maritima, qua seminibus punctulatis nigris gaudet, videtur diversa." If this is not a blunder of mine, the new species must be placed in Chenopodina.

My Atriplex consists of A. latifolia, Wahlenb. (A. hastata Auct. non L.) and A. littoralis, L. "foliis lineari-lanceolata nunc dentibus arguris paucisseriis (1-2) instructis. Spicæ terminales strictæ clavatæ, foliaque et caulis plus minus lepidoto-incana." Iris gracilis or prismatica, is referred here to I. Virginica L.

These instances will perhaps shew that I have not been altogether a stranger to the interesting enquiry opened up by you in Silliman. Some of the mistakes do not illustrate this, and may be worthless to you, but I could hardly forbear availing myself of the occasion to let them come to your notice, as I well know that at the time they were written, I believed them to be accurate. Still all this does not incline me to give up any of my respect for Potam. lucens, P. rufescens, P. pulcher, and P. amplifolius as natural species of Potamogeton. Backed as to P. rufescens by an army of Europ" observers, I feel perfectly sure that it is distinctly a natural species. I say the same of P. lucens, as another long-known plant. And everything I know of the other two (I have P. pulcher from N. Engd to St. Louis, & have traced P. amplifolius from Fresh Pond thro' the Lakes of N. Y to Niagara) is in favour of their distinct specific character.

To refer back once more to what I said about giving credit in all cases to the first discoverer of peculiarly rare species, I would say that as this is usual with your work, and certainly adds an interest to the diagnosis, as it is a deserved remembrance of the finder — I can furnish you with several others which are unnoted. Dr. Pickering was noted for his keen eye for novelties. Oakes carefully mentions in his printed lists those of the alpine & other rare plants which were (in common) of Pickering, and I remember among them Carex prælonga a singularly interesting stranger, which as truly belongs to Dr P. as CC. fulva, lævigata, & Salina do to Mr. Greene; or Lupinus to Oakes, or Rhizanthus to myself.

At p. 584 I see an error in the Synonymy of "C. cylindrica, Schweinitz," which makes the C. Muhlenbergii of Dewey identical with that of Boott, which I wonder at. Your C. cylindrica is Dr. Boott's C. Muhlenbergii, but Dewey's is your C. bullata I am sure. At p. 587, C. rigida, var. Bigelovii, was established by me in Sill. Journ. on just the characters given.

At p. 595 should not "gracilens" have been printed "gracilescens"?

p. 43, Vicia villosa (that is "V. sordida," Auct. Amer.) & Jordan's Elect Comb[ae] according to my Essay. I remember it as like my other Spec", a, bellorum. cucullata & sagittata ovata.

p. 117 I am sorry to lose so old a friend as Gaura Peckii, but I have long been prepared for it. Mr. Oakes would have mourned over it.

P. 118, Potentilla frigida. The station omitted. Robbins first found this, past flowering, at the locality on Mt. Washington. I added the Mt. Lafayette station, and according to Oakes was the first to gather it in flower, the color of which was before unknown.
P. 134, Myriophyllum scabratum was one of Oakes's most valued discoveries at Plymouth, in 1839 I think in my paper
p. 281 Aphyllon uniflorum. There is a B, major, Mihi Enum. ms. with all the parts larger, anthers twice as large, stem quite puberscent and flowers ochroleucous, which I found near Cambridge, and mention for what it may be worth.

But it's time to stop these petty criticisms which do not touch your great work. Still I may have some more to offer at another time. Thank you for the beautiful plate. Indeed every Botanist must be thankful for such a vade mecum as this.

If in the above I have said anything which you do not consider sufficiently calm, be so kind as to set it down to my interest in the plants, & not to any (intended) carelessness of expression. I hope some part of this sheet may be acceptable to you — & am sorry they have not more in them that is intrinsically valuable.

Remain truly yours

Edw. Tuckerman

To Professor Gray.
Cambridge.

P.S. Do you think I had better continue my investigations of Potamogetons, with a view to a Monograph, with plates?

Amherst 25 Nov. 1836

Dear Doctor

Your long friendly letter was duly received & appreciated by me. I am able to conceive of the little time you can have for such writing. Thank you heartily for it.

I have really nothing of any importance to urge further in regard to the many topicks referred to in it, being either superficially content with having engaged your attention to them, or satisfied, at least pro tem., with your disposition of them.

It is exactly because I have a historical taste that I like setting up and keeping set up the little monumental pillars which in all local Floras of a classical kind, have been assigned to the discovery of species — but I am aware that your Manual is hardly a local Flora, but something at once more extensive, and also in some respects different.

It is interesting (to me) that Rhinanthus has so wide a range as an indigenous Amer. plant, but still I won the print, and let me say that M. Baker's

name really ought not to be associated with mine in regard to this plant, as he never saw it till I brought it to him, & never found it till I showed him where it grew. I found nothing at the White Mountains in company with him. Lobelia ? also exclusively my find, and it delighted him, you will believe, when on my return from Franconia, I gave him the first American specimens he had ever touched. But enough.

As to Jorelya, I undertook last winter to prepare the book for a reprint by the American Antiquarian Society, and to furnish it, to this end, with the needed notes, so far as this was in my power. It had long been my intention, and I hastened to carry it out, when the Society honored me with an election. The manuscript is now ready, so far as I shall attempt to make it, though here in the Reading Room of the British Museum, or in Pamplin Slop, I could add much of interest, and only wait for the preface, in which I mean to touch on some bibliographic matters which I have not got fully looked up. There are beside one or two difficulties which I am going to trouble you with, if you will let me, and all this must be done this winter.

As to the other plants not mentioned by Jorelya (that is, the Weeds generally) I attempted, in the "Enum. Pl. agri Cantabr." referred to in my last letter, to refer after every plant to the year in which it had first been made known as a member of our flora — this one of publication being the only available one however uncertain in many the year of discovery, and I added always the name of the first publisher, without regarding who was first discoverer, unless both were the same. My list therefore gives a number of weeds with "1669, Jorelya" after them; a good many with "1785 Castle" attached and the rest with "1814, Bigelow"; "1824, Bigelow"; "1835 Pickering"; & so on with "Oakes"; "Tuckerman" &c;

I hope you may find my reprint of old Jorelya sufficient to answer your purpose. I fear I cannot furnish either, I am, further discussion of the question about the history of our introduced plants, now, at least for would be impossible to get the exact date of the first observation of Carex praecox, paniculata, &c, either from Dr. Pickering, or Oakes's papers in Hovey's Mag. and it: the last probably which furnished the dates I find in my manuscript Enum.

But I must close, or feel that I have tired you out. I know not who but you could either commence or continue the pages in question. They are very interesting, and I wish my duties as Professor did not engross my time for this winter, that I might enter on some one of the interesting inquiries as to our Weeds to which you refer. I am continuing the perusal of your Manual, with a view to endeavoring, agreeably to your invitation, to find something or other to offer you observations upon.

Thanks for the books & plants. I will send any books for Mr. Faltoner, to you, by Buck, when I come to Boston. Thank you also for the news of any book being discovered. I shall see the "New Mirabeau" when in Boston. I do not now take it myself.

Believe me Truly Yours

Tho. Jackman

To Professor A. Gray.
Cambridge

Amherst
2 Feb. 1857.

Dear Doctor

If at any time in the course of your correspondence you have an opportunity to make any enquiry concerning either the forwarding from this country or the receipt on the Continent of any Lichenes Exsiccati, fasc. III & IV., I shall be very glad.

I have had no intimation that one of the volumes has reached the continent & Schlechtendal has not noticed it in Bot. Zeitung, which I have to Dec. 19th.

The same is true also of that valuable set of Venezuela & West Indian Lichens which I sent to the Smithsonian Institution for Dr. Montagne.

I will not trouble you to write, but shall be very glad if you

are able at any time to attend to this in the way of your correspondence.

It is a little discouraging to have no acknowledgments of so many parcels & what I never met with before.

I find among my books a thin octavo entitled "Tracts relative to Botany translated from different languages", &c. Lond. 1805, which contains "Observations on the Genera Juglans, Praxinus & Quercus", by Muhlenberg, from the "Neue Schriften d. Berlin. Naturforsch. Freunde." If this is not in your library, I beg to offer it for a place there. It cost me but a trifle at Pamplin's, & is of no value to me now.

The continuation of your paper on American plants in Silliman has greatly interested me — Thank you for the kind reference to myself.

I wrote to D^r Liebmann of Copenhagen, on receiving a letter from him telling of Vahl's death, and offering me in the pleasantest manner a set of his Mexican Lichens — but now I find from the Bot. Zeitung — that Liebmann is dead too. I had anticipated a pleasant & profitable correspondence: though indeed he was sick abed when he wrote to me. The Copenhagen climate is terrible for it's piercing winds.

I am yours sincerely

Sam^l Sullivant

To D^r Gray.

Amherst
13 Feb. 1857.

Dear Doctor

I want to beg of you a copy of the second part of your Statistics of our Flora. You sent me the first, which is solitary without its companion. As you mention a new issue of the Manual I just note here any "Errata" from the fly-leaf of my copy.

P. 116, Geum strictum (Ait.) Is this also European?

119. Potent. tridentata reperies (Ait.)

62. Amychia dichotoma "Common throughout," struck me as incomplete, but on second thought I know not if it be.

– 66. Etymol[y] of Calicotome wanting;

– 128. Amaranthus familis, Massachus[ts]. B. below.

132. Juncoides — In Etymol[y] observation omit "as"? !

– 134 Myriophl. scabratum, Plymouth, Cape, & York discovered in this part of the century.

– 139 Sicyos angulatus, most common with us i. e. about Boston, as a weed in cult. grounds about houses.

– 155. Ligusticum Scoticum, Cambridge, &c.

– 168. Viburnum pauciflorum, High Mts of Vt.!

204. Solidago Muhlenbergii. Base of White Mountains, Gray! teste ET.

225. Galinsoga. I was the first to find this & I only found it in a Boston Common, where it soon became extinct.

231. Senecio vulgaris. Pa. (Muhlenb.) Darlington.

249. Vacc. uliginosum, I shd call a subalpine, and by no means an alpine plant. Am I wrong as to this?

257. for "orthography of the names" might it not be better read form of the name, or spelling of the name?

— P. 309, line 4, omit "and".

P. 325. Gyroglossum Morrisoni — I am afraid you will not agree with me, but I wish you would leave this poor plant without any other (metaphorical) Stigma than its vulgar name. It is not vile to me.

347, under G. Peberula, correct "Seeds (also in G. Praemorantha) &c

— 374, Polyp. auriculare littorale, Gravelly beaches of Lambrigde bay!

Why — 390. "gracilaus" — sh'd be gracilescens?

Juan — 406 — 2. falcata, "valualle" for variable?

Thus I have told you all that is noted in my copy of your classical work. Whether you agree to all or not, believe Me that I have just thus done, only to do what I can to advance it's growing perfectness — even in these little things.

Thank you for your last note. I received one from Dr Vanden Bosch: & a long & very pleasant one from v. Martens, which repaid me fully for sending him my Book.

Yours truly
Wm. Tuckerman

P.S. I have just been looking through (in a Bookstore) the last edition of Mr. A. Woods' Book — & am utterly astonished at the outrageous way in which the Spelling of the names is disregarded. I sh'd think there had been no corrector. It must make mischief, in this way if in no other.

Amherst
9 May 1857.

Dear Dr Gr

I thank you heartily for the Lichens. They are all Texan species, except one, which is new to me from the Southward, though apparently (I have not yet begun my Botanical studies for this season) not different from a northern form of Stereocaulon. The specimens are very fine & valuable.

Montagne has sent to me his notes on Fendlers lichens. He names a fine Stictа after him, & also an hepatic moss which he observed growing upon the foliaceous lichens (Ragiochila Fendleri.) Knowing it would be quite unpossible for me to elaborate these plants except in Europe, that is, as well as they could otherwise be done — I desired Dr Montagne

to consider the specimens sent (all of more difficult ones) as entirely at his disposal. Some of them prove of great interest, & I look with much interest toward the next remittance. The difficulty of proceeding, with tolerable certainty, in determining tropical lichens, away from the European collections, is obvious — but I hope to be able to do more nearly with the next parcel.

My wife is studying your lessons in Botany with great interest, both in it, & the plants it introduces her to, and I study it in order to hear her recite, & feel every lesson how much it contains that I have either forgotten, or never knew.

Did I tell you that the 42nd edition of Wood's Botany is on sale in the Bookstore here? It is positively a shame that such bare-faced fraud should be permitted without rebuke.

I am truly yours

Edward Tuckerman

—2—

Amherst
10 July 1857

Dear Doctor

The Mosses came safely
& I shall acknowledge them by a note
to Dr. Hooker. They are all Northern
forms, & I notice but one that does
not look familiar. Indeed I have
no doubt nearly all are identical with
our forms — Most Some, I am aware,
have received names as distinct, which
they are 1) geographically, & so 2) in some
degree, as might be expected, really or
physically. The whole aspect is
entirely familiar — as we read the Flora
of the alpine region of the Himalayas
is generally. I prize them much,
& shall rejoice over more, & intend
to ask for them when I write.

I have begun a little collection for

Sir W. Hooker's Economical Papers — but have nothing yet, but a good sample of our chestnut wood prepared for the "finish" of houses — & a mat of Witch Hazel (new to me) which is however not yet arrived.

Thank you heartily for these. I enclose a dollar for charges on this & the last, which perhaps covers all. If not, I will pay it hereafter, that I dislike to give you the trouble of recollecting such things.

Believe me
Yours Sincerely
E.W. Teschemacher

P.S. And I hope I have something which you will not be unwilling to put among the curiosities of your library. Indeed I thought you had it, till I found it in my portfolio the other day. It is a MS map which I found in the parcel of P.L. Panchaud, bought by me at the Lambert Sale — & doubtless by F. Pond lately — I do not like to risk sending it by mail, but will bring it with me to Boston, when next I come, if you will take it. ET

Amherst
28 Sept. 1857.

Dear Doctor

Thank you very much for the interesting parcel of Lichens, which reached me this evening, & has just been glanced at, though I must defer examination till day-light. I will do as you desire with them, & most gladly accept any part.

As to the Plates, — you will not perhaps be surprised if I say that I feel very little acquainted with them now; and in consequence a little anxious about them. It is so long since I have seen either the text or the Specimens — that I

and I will then sit down with W. Sprague to the elucidation of their Spores; & also arrange the ms. for the printer.

I really think, as the Government has kept back my manuscript so long, & has denied me all access, without an expensive journey, to the Specimens which I elaborated — that it ought to afford me every facility for such collateral emendations as I may be able to make in my list — & am sure that you will accord in this view. But I will not come till November, unless you insist upon it

Yours truly
Dr. Tuckerman

P.S.
Thank you too for the Sheets from your Work!

must confess present ignorance as to "which is which".

I did the work when it was put upon me, as well as I was able; — & I revised the whole while abroad the last time — which is now seven years past — & since I have not seen any thing of Specimens or manuscript.

I even was compelled to return the least bit of the Specimens, so that I have nothing to refer to.

I will collate my list with the Flora Antarctica when I come to Cambridge — and should be glad to engage (as you suggested) Mr. Sprague to illustrate the spores of the new species in a single Supplementary plate, which I suppose will not be very expensive to the Government — In that case the Specimens of the new species should be furnished from Washington — and all the Specimens — as the apothecia do not always furnish perfect or mature spores.

I am most happy to be acting herein under your direction, and shall do as you think best — but if not otherwise instructed should incline to leave it till I come to Boston in November, at Thanksgiving. Would it not be in season? The Specimens might be procured meanwhile,

P.S. I open this to enclose one dollar with which I beg you to have been paid for my postage, Express, & other charges. If not right, I will make it so.

Amherst
10 Nov 1857

Dear Doctor

I am in possession of Mr. Wyeth's precious collections! I have not yet glanced within a single sheet, but know very well what is to be expected, when he is collector. I only trust that I shall not have to go to Paris, to make out the varieties that I shall find. From work of especial importance as my place is a new one, and many important things are to be done once for all) still keeps me from my plant-room; and I do

I am glad you received the Apples. The year is a bad one for keeping Apples. Mine decay rather badly — & I fear you will not find yours without many defective ones. Another year we shall hope to ask you to try some better ones & other sorts.

I call my place Appleshead. I have a hundred trees in bearing & have added many choice sorts already, & mean to get everything in the shape of an Apple that is quite first rate. But I am first to bring the old trees into good condition, which is hard by work now.

Believe me
Your friend
Edw. Tuckerman

not expect to get at study till the week after Thanksgiving: when there will lie before me in one earth treasure-heap, — the largest and most interesting set of collections that ever greeted the eye of any American lichenist. I hope to do it tolerable justice, but I had rather avoid all avoidable chances of mistake, — and with therefore my Schick Microscope wd shew me the spores a little more distinctly than it does, & that I in short, was in Berlin or Paris to set all right in that regard! But we shall see. I shall gladly accept of a full set of specimens for my herbarium, & not think of any further compensation for the pleasurable task of studying these treasures. As to the other sets, I do not see how a better disposition could be made than the one you suggest. Eric ought to have one; not for any use it will be to him, for I do not think he will work more on Lichens; but for his Herbarium of Lichens, & his son, who I hope will bring out some day the promised new edition of the Lichenographia Reformata, under his father's eye.

I shall keep your note by me for reference to all other points not referred to here; & will govern myself accordingly.

Amherst,
3 April 1858

Dear Doctor

Thank you much for the information contained in your letter, I have sent my parcel by the Amer.-Eur. Express company — & inclosed your packet of Specimens, which arrived safely. It is my wish that Mr Flanders should have my Specimens, which are generally ample — & retain duly for himself. I have sent some however expressly for the Mus. Parisi.

Thank you for the Subscriptions of yourself, & M. Eaton to his work.

them if you have time to make it out", adding that he wished to append the List to an account of the Plants which he was preparing. I write to tell you the circumstances of the case, as otherwise friends might wonder why the Mosses should recieve so much more honour than the Lichens; there being no apparent reason for the difference; as I was as able and willing to tell what the Lichens were, as W.S. was to say what the former were! But don't let me induce you to think I have any wish to complain. I shall furnish Dr. Torrey with the list he desires, which is doubtless intended for a future volume of the very remarkable work in question. I only cared about it, because I had the materials, and could have elaborated them.

Ever.

P.S. 2d: I ought perhaps to have made the list out at once, last August, but did not suppose there was any hurry about it, & prefer to do such work in cooler weather on account of the heat of my plant-attick,

I have so far but 5, but may probably reach 7 — should like to make it 10! I propose to have the copies sent to some bookseller in Boston, for distribution.

What do you think? The Washington Ex. Ex. lichens have come — & there is not among them one of the species or varieties which I described as new. Half a dozen genera are indeed wanting — but I can only account for all the curious things having been sifted out — by the supposition, that when W Breckenridge looked them out, to break off bits for me to take with me to Europe, he left them apart from the rest. I wrote at once to W Vasey the Curator, & suppose I shall sometime see the rest of the collection.

Sincerely yours

Edw. Tuckerman

P.S. In looking over the ponderous reports of the Pacific RR, the question was asked why the lichens, of which I possess a complete set, were not also catalogued, — & I had to say, that I did not know, except that I had never been invited to. In a rather recent letter Dr Torrey says of a collection of some 3 years since, these lichens sent to me by him, long after I had received a set from Wright, (which collection from Torrey, was, through some oversight, sent without any letter accompanying, so that I had to write, to know what to do with the parcel, as there was nothing in it, not already in my possession) — that he would greatly desire your opinion of them, and a list of summer 1857,

Amherst
8 January 1838.

Dear Doctor

Thank you for the opportunity to read Dr. Hooker's pleasant letter to yourself, which I herewith return. I think they do well to have Nylander go through their Herbarium of lichens. I have just received his Lich. Exsiccati from the neighbourhood of Paris (150 nos) the last copy, which I got through Pamplin — & it is fuller of interest than any collection I know, excepting only Fries's. If he is really at work on a General Synopsis — which I only guess from Hooker's letter, never before having had any intimation of it, I shall certainly be ready to help him in all reasonable ways. If you can get a hint, to this effect to him in the course of any of your correspondence, I shall not be sorry. I was long since put on my guard against him, & have

Reading kept him off, as a hunter after new species — but I have now seen enough of his abilities, to be willing to do my part in aiding him in what he is well able to make — a new General Work on Lichens.

I also wish to thank you for the Catalogue of Jussieu's Library, sent & supported by M. Decaisne. Did you send me more than one other foreign pamphlet (a report by M. Fenzl on the colouring properties of a lichen, which I am right glad to have) — I conclude not, & beg you will not consider this as a question seeking an answer.

I mean myself to write to M. Nylander, & send him a batch of my crucces as Fries calls them. He seems to enjoy those things the most, & has made good plausible work of some which Fries gave up as not to be dealt with satisfactorily.

I am just going to attack the splendid collections of Wright.

Yours Sincerely
Edw. Tuckerman

To
Dr. Gray
Cambridge

but I suppose hardly at all the common ones. I have two or three sheets of Oakes's mem°, which do give the common ones — rare generally I doubt not — but I find on exam° & comparison not always reliable. — It will be worth a great deal to my hen if M. Sullivant will find such a List — of Hepaticæ as well (so far as known —, and possibly the Fungi may be managed to form the nucleus of the Synopsis of Northern Fungi, to which you look for Carter's attention I believe, for the Manual.

Mess^rs Ivison & Phinney favored me with the opportunity of expressing my admiration of the Manual, & I did so. They promised to send me both the new edition of that & the Lessons. I promise you I will study the last, when they come.

I find one or two more errata marked in my copy of the Manual:

p. 379, Saxafras. "Especially Eastward"?

399. for "Wittemberg" read Wittenberg.

405. "common throughout"? ("the expression?")

My Catalogue will rest now till I can get through some other work. I hope for the progress of your work as for the progress of science & station I can do (this little) I will. Exactly. No Charlatan's compilations shall take their place if I can prevent it. Yours truly

Edw. Tuckerman.

28

Amherst
3 March 1857.

Dear Doctor

Potentilla tridentata is a rare Scotch plant according to Hooker Br. F. 1. 253, & he cites E. Bot., tt. I believe it was first discovered in Newfoundland, but that is not so odd as that C. fulva, which is so nearly unknown here, & so very common in the North of Europe, should also have first turned up in N. F. d.

Vaccinium uliginosum is certainly an alpine plant with us, but I suppose that it attains to its perfection in what we might designate our subalpine region — being rather dwarfed above, & ceasing, except sporadically below. The Loiseleuria, Phyllodoce, Rhododendron Lapponicum & Diapensia belong to a range, which does not indeed exclude Vaccinium uliginosum, but which they cannot leave to follow the latter downward. The only approach to this which I have noticed is, that on Mt. Webster, (the long Spur which

from the Eastern wall of the Notch pass, over against Mt Willey — that is on the very tip of Mt Webster, there is a slight indication of a small base subalpine region, which Lecidea geographica attracts attention to, & Diapensia almost determines — & here the last might be considered as extending the range of the Vaccinia; but such instances are not common, & I believe the latter is always best developed below where any of our most alpine plants can be found.

It descends the stream & the base of the mountain and continues there often plentifully as at the Flume in the Notch. I find it set down in my one of the Note Books that on one occasion in 1840 I went out with another to pick Blueberries a the side of the Notch hills, & got 8 quarts in the afternoon, pastures of V. Canadense — and that on this occasion the V. Pennsylvanicum about the Flume appeared to me to have come down from the higher regions, to be just as strange there as V. uliginosum: It is perhaps more common than the latter in the alpine parts.

I am right glad to hear of the success of Wright, & Tuckerman. I wrote to Mr Wright as you suggested, & as I was very happy to do.

Montagne visits 6th Feb. acknowledging at length my book, & he stops his letter to acknowledge also the Venezuela lichens, which charm him exceedingly, as I am sure they would as he has elaborated the Swiss forms.

He promises me some books by favour of an opportunity afforded by M. Lasegue's standing by you.

I have often been desired to prepare a Catalogue of the Plants of the White Mountains — & the other day I took my Note-books, Robbins', some Mem° of Oakes (They are scanty enough) & put down everything I could find. The result is a tolerable view of the Flora. There are some interesting plants as Pyrola uniflora, Dicentra & which I received & for which I hope to be permitted by the present possessor of Mr. Oakes to consult the herbarium — if I do not myself go and hunt up the localities. But do you think that M. Sullivant will be willing to print me, under his hand, with a mycology to include all the common as well as known species? His Synopsis gives me a goodly number

Amherst
25 June 1857.

Dear Doctor

I wish to tell you at once why so pleasant a note as yours of the 12th May! has been left unanswered till now — before I thank you for the noble pleasure of the 20 June, received yesterday. The first arose when I was in the course of my historical lectures to the Seniors, which this year took up my whole time for several weeks, and it included questions which could only be settled by consulting Montagne's letter (which both the Bench, & its place in my working-room in the attick disinclined me to,) or rather writing to him at Paris & getting his reply. So I postponed the answer till History was despatched for the term, as I did all my other correspondence.

But I know you have enjoyed me by your kind messages of this week.

Mr. Tuckerman continues her Study of the Lessons; & I should emulate her

(for I am well aware I need to, enough) were it possible for me to pretend any longer to other than such knowledge as I have of the past of Botany which I continue to study.

As to the paper on Venezuela bodies I should like to take just the course you suggest. Montagne's notes are sufficient to show me that there is probably a good deal that is new to be expected from Venezuela. But I must write to him for his descriptions of the species which he has named. He offered either to send them, or to print in Paris. When they come I will prepare the paper at once.

I should also be delighted to study the Lichens of C.B.S. Japan & Behring's straits which you mention. I am not absolutely unacquainted with the first named, though at present our knowledge about them is a sad medley of herbarium-names & guesses (all sorts as well as some accurate judgments — and the last will look more or less pomilio). Mr. Cuba[illegible] collections will be very precious, and doubtless illustrate in cyclus what some extreme point of your South coast shows here & there indications of.

Nepalese bodies, as well as [illegible] species, both from Sir W. J. Hooker, Greville & others, which there will probably enable me to determine.

Yours sincerely

Dr. W. Tuckerman

Amherst
24 April 1858

Dear Gould

Thank you very much for the Plantæ Junghuhnianæ. It seems to me that I ought to have it from the author. M^r V. d. Bosch writes Dec. 29, 57, of some lichens from Columbia of which he promises to send me, "together with it you will receive a second parcel of Lichens" & "For the first is just now despatched, together with books M^r Gray wanted, and which I had the good fortune to secure for him. It contains all that at present was disponible of Javanese lichens." And D. Montagne writes 18 Oct. 1857, "J'affranchis en même temps que cette lettre notre travail sur les lichens de Java;" & closes me in a postscript to acknowledge the receipt of this letter, "et surtout des lichens de Java." Both have so far failed, though I feel safe about the parcel from V. d. Bosch — the other having probably gone through the Post, & been wholly lost. I will with your leave retain the copy you kindly send a little longer, & see if I do not get one myself. It is quite interesting, though I know nothing specially of the relation in which the Java

Lichen-Flora stands to others with which I have some acquaintance, & cannot therefore judge beforehand with much satisfaction how far the new species are likely to extend.

I am unacquainted with the management of copies of foreign works, & shall much prefer to have the Nylanders sent to Westermann. Thank you for the suggestion.

The Washington people have sent another parcel containing as they say, (but as they spell very badly, I know not how far they are otherwise reliable) all the missing species, excepting two — perhaps the most interesting, & the new species, which cannot be found.

I shall, in a day or two make a second revision of the whole collection and report all that are missing; that there may be no misunderstanding hereafter.

I am just through (except posting in tickets, which are also to be provided) the VIII Fasciculus my Exsiccati.

Excuse this bad scrawl from

Yours sincerely
Edw. Tuckerman

N.S. I shall be Dr Boott's guest while at Boston.

Mr. Asa Gray
Cambridge
Mass.

Amherst 31 May 1850

Dear Doctor

I should have written to you before this to say that the Memoir of N. E. Bosch & Montagne on the Lichens of Java, turned up, among my pamphlets, which, as we have no Bookstores here, occasionally accumulate, & that I will therefore return you with many thanks your copy of the rather Plantae Junghuniana, containing it. I had no doubt that your criticism in N. E. Bosch's expected paper was much more than my poor, though I feel quite willing that he should send me as many Lichens as pleases him:

The article in Silliman, was a Supplement, not to my Synopsis of Lichens of the Northern States, &c, which was published in the Academy's Proceedings, but to a small separate work entitled "an Enumeration of the Lichens of North America, which I published in 1845, & in which I added, if I remember right, some 70 to what were previously known.

I gave the new paper the name of a Supplement to that Enum., as the best I could do with it, and I published it expressly in order to get descriptions of the species into print, as I proposed sending them all to Dr. Nylander. There were one or two especially, I seem in which I had a peculiar interest, specimens of which I knew he must have seen in England, & which I did not doubt he would anticipate me in naming, if my descriptions were not in print as soon as possible. As it is, he came out with an Enum^n of all Lichens in a small brochure, just a month before my article appeared, & one of the species above-referred-to, is, I have no doubt, taken up in it, — though as there are no descriptions in this "Enum" — mine will perhaps hold its own.

I can always tell when the next N^o of the Ann^s Journal will appear, but (perhaps this my fault) I do not know anything about the publications of the Academy, now I am so far away, nor have I seen any Proceedings for two years. I enclose my obs. on the Lichen from the Platte, which like many other lichens is surrounded by a beautiful halo of obscurity. The facts I have given are however interesting, and this unquestionable, I suppose, that nobody else knows as much as this, the plant having disappeared from the authors, since the time of Swartz & Acharius. The former seems to have been in New England, & to have collected few lichens, & this he says was one, which I cannot but doubt, so far as the West^n lichen is concerned.

There are one or two of the new species (one, Sticta Parkeriæ perhaps as interesting as any) entirely absent from the Lichens of Wilkes Exp^n. — also Parmelia Pourouleria, n. sp. which it is important to have the spores of described — & the whole of 4 genera, are wanting. A few have been sent to me, since I first received the Lichens, but all the above are still wanting. I shall in a few days report the state of the collect^n & see if the rest cannot be found. I have revised the whole work, & can put it in form to print, as soon as I get the missing lichens, which ought to come under the same revision!

Messrs Jordan & Phinney have not yet remembered me, but I hope they will; — and Master Wood is not yet, as he ought to be, banished from Amherst. But he must be soonee or latee. I am glad to learn that D. Booth work is on its way.

Will M. Wright let me tell him that you that the Nicaragua lichens are beautiful & peculiarly interesting — so far as the first glance goes. —

The Bething Trails have been done some time, but I sent out two or three about which I could not make up my mind — to Paris, & have not yet heard. I would thank him for his note & these new proofs of his genuine research. With my Best Thanks to him — Jamain Yours truly

Wm. Tuckerman

P.S. The panels of Lichens (Antarctic & Others from Hooker) from Jom Herb^m have furnished me some precious bits. & some beautiful Species of Lichens not represented in my herbarium. I will return the set when I send Wright back My Exsicc^a Vol VII Fasciculi or Tom. III, will be ready now in, say 2 or 3 weeks, when I will beg Smith me to pro-

Amherst
30 June 1858

Dear Doctor

I am very sorry to give you so much trouble. It's a great thing for me to get the Parcel to Dr Nylander — & I despatched it this morning, I hope safely to hand. The arrangements of the Academy for publishing are very liberal & handsome & I shall be happy to avail myself of the opportunity to get my Lunar-publication. They have proposed to me to take the Chair of Botany here. At the time I came it was filled by the Prof. of Chemistry, who desired to keep it. They will give me a sufficient stipend for

my annual course of lectures & I shall of course enjoy my present post, which is not salaried, & takes off any invidious shades. So you will send Amherst students in future! I enjoined the fact. Even frankly, that in my opinion they had better get a young anatomist — who could manage anatomy & Physiology at once — I was familiar with dissections & Physiology — but no doubt I can begin even now the two courses of instruction — & I intend to do so. The College Enclosure is started — & shall be continued. Here there is rather a large Cabinet of Fruits, Seeds, & Sections of Wood — but I am by Physicians. But this arrangement will not come into play till next year — if then; so I will say no more at present.

Yours truly

D. H. Hitchcock

P.S. I prepaid 37½ on the parcel, beg the charge to Boston — but will refund all further charges which ought not be accounted for.

valued by me. I can buy the books with me to Boston at Thanksgiving. If you do not reply — I will consider that I may send them to Prof Henry.

Yours sincerely

Tom. Tuckerman

Prof. Gray.
Cambridge

Amherst
7 Oct 1838

Dear Doctor

Let me thank you for two kind notes. I have all of Nylander's publications mentioned by you, except the part of his Synopsis — and ought to have that as he wrote me (2 Sept.) that he had deliv'd the copies for Amer. subscribers to the Western Agent in Paris. I must therefore decline to avail myself of your kind offer, and as Dr. N. has sent to me (he writes) a copy of his monog. of the Pyrenocarpi, for sale, I am afraid I can hardly dispose of a second. He is a splendid worker, and his book will prove to be a necessity with every lichenist whether he will or no. I shall gladly prepare a brief notice, as you desire, but think it should depend till the Synopsis comes, as that will be the main thing. Will it not do as well for the succeeding no of Silliman?

I am very glad the Lyellian pleases you. It is a good volume in several respects, but I do not know anything particularly noticeable, except it be the large proportion of species of the difficult genus Cladonia, which are cont'd in it — & also in

the first two Fasciculi. There is scarcely anything in the new volume that will be so interesting in Europe, as the curious aquatic cellulacean plant which makes № 150, Hydrotrya of Ruffell (in Trans. of Linn. Soc.) Nylander heartily accepts the genus, which differs microscopically from the older genera — & most remarkably from all other species.

I have had the Manual with me all summer — & have found a few new errors of the press — as follows: — p. 542, Vilfa virginica, "leaves compound". p. 650, "Lenkea" (Lenkea?) 664, "old bogs" (old logs?) 565. Festuca ovina wants the author's name. 549, Triticum repens — Is it not indigenous in the alp. region of White Mountains? I have the impression, but no specimens — Think they are perhaps in Herb. Oakes. 541, Phleum alpinum does not occur on the "tops", nor does scarcely any other plant, beyond Carex rigida, — but in moist places of the ravines. 541, Alopecurus geniculatus is common at Boston. 423, Poa debilis; I carried Mr. G. B. Emerson, a cone from the highest region of trees on the White Mts, which he carried, as I did, to be this species. Last year I sent the Keeper of the Tip-top House who has made a little book of the curiosities, a List of the Alpine Plants. This I have promised to revise — and do more at length for another edition — & paid some attention this season to habitats — or native stations of all the species that occurred to me. I mean saw Oxyria, and Graphical Lapponica so abundant as this year in Tuckerman's ravine. The former also occurs in the bed of the stream down the ravine, at an elevation of I suppose not more than 3000 ft above the sea.

When you write again, will you give me any direction for sending my copies abroad. I did not receive acknowledgments of near all of the last vol. but suppose they must have reached their destination. Will the Smithsonian take charge of them? I have yours to send to Munich — A word will be much

Amherst
14 Dec. 1858

Dear Doctor

Thank you for your kind notice of the Lich. Exsiccati. It has had the agreable effect of procuring me a purchaser (a Mr. Poe of Montreal, whom I do not know) to the 4 Nos and the preceding volume.

Mr. Wright's Lichens so fully occupy me that I have not been able yet to put my thoughts in order for a notice of Dr Nylander's work.

But I have just got Thuj-

the lichens. In this state of things, it will be far handsomer, & select from the plates only, for a single one — instead of trying to have two. Can this be done? When these plates were drawn, I was utterly inexperienced in such things, and they made no application to me for direction. I really think a single plate will be more creditable, than the two as they stand, especially as I do not know what to call some of the figures.

I will bring you the MS. for your criticism — at the 1st of January. Meanwhile a line will be much valued by Yours sincerely

W. Nylander

any work of necessity
of the Report on the Exploring
Expedition Lichens — and before
I send the Specimens back, I
wish to ask, for one time,
if it is entirely unobjectionable
for me, without special per-
mission from Washington to
retain specimens of the Duplicates
are most desirable *.

I wish specimens only
a part, and of nothing of which
there are not numerous duplic
cates. If it is desirable, I need

* I understood you to say this some time ago.

not say to (you) that there shd
be something for me to turn to
as what I have named to & so
— I am sure you would thank
to — If would have
saved some trouble, if I had had
such specimens from the first.
I shall be very glad of your
judgment in regard to them.

The Plates are very unsatisfac-
tory. Some mistakes were made by
the Draughtsman. Some things are
figured which there was no
reason to figure — & there
are two or three figures, which
I cannot appropriate to any of

Amherst
2 Jan. 1859

Dear Doctor

I thought (I feel sure) that I wrote Dr V. D Boardman & told him I should be glad of some more of his Lichens — but shall do so again.

Mr. Thaxter's letter which you kindly inclose ought to satisfy me I suppose, as you consider him a peculiarly qualified judge — but it (I must either confess having an opinion, or have one) does not.

If he had said it was a state of something which I did not know, I should submit — but he tells me it is something which I know

and knows, as something else.
But I will study it again,
with all my attention — &
fully mean to try & hard to
see the thing as you do.

Thank you for information
about the Lichen Report.
I am about two thirds
through with the North Pacific
Lichens; but the remainder
are the worst of the whole
(the Peziza & Sphæria) Jons)
& will give some trouble. I
long to be through it.

If you ever have any chance
to say to M. Stuart, that I
should be glad if the fifty dollars
which they engaged to pay me,
it will be a favour. He said
Vouchers of the services would
be required — & the money will
now be particularly acceptable.

Your ever obliged
Edw. Tuckerman.

With Mrs Gray's journey accept
my Best Wishes at this happy
season!

Amherst
7 January 1839

Dear Doctor

I could not help, feeling when I got away from your study the other day, that I had to it thanks for half enough for the light and help you gave me during the hour I was there, and so I write to do it again.

I send today, by Thompson's Express, prepaid, a box containing 12 copies of Luther's Sermons for foreign correspondents, which you are so kind as to send for me. I wish I could do something in return.

The revision of my Report
on Lichens for the Wilkes Exp.
edition, will be begun on
Monday, and finished as soon
as possible. I am glad
to know now exactly what is
wished - as to form.

In haste your obliged

Edw. Tuckerman.

Löchern Amer. Exs. E. Tuckerman

for Wm. Borrer Esq.
R. K. Greville Esq.
Sir W. J. Hooker
Jr. Boott Esq.

and Dr. Klotzsch, Berlin
Dr. Lehmann Hamburg
Prof. v. Martius Munich
Dr. Sendtner Ratisbon

and
One Packet for
Mr. von Krempelhuber
Munich

E. Tuckerman.
6 Jan. 1859.

Amherst
26 April 1859

Dear Doctor

It is so long since I have heard from you, that I will no longer delay writing. My last was in response to yours about a Potamogeton, and contained the specimen sent, (as that (at your kind Suggestion) I kept.

I hope you and Mrs. Gray continue well, and that you will give me a line, if only to say so.

Dr. Asplander's anxiety also makes me wish to write. He says (29 March) that Tulasne has written. To him I'd say that no parcels have arrived addressed

do nothing else till this done. I finish, at the same time, a report on every genus, as it comes up, as represented in North America — So shall have a full new Enum[n] ready when the other Report is. I could not make the first Report, without first that general Study which makes me all ready for the other — and I find a great deal of satisfaction in thus bringing together & concentrating the whole that I know, upon every point, 1st as respects North America, & 2nd as respects countries visited by the Exped[n].

Sincerely yours

Edw. Tuckerman

to him, those others have come (those for you) addressed both to the Museum, & to Dr. Montagne.

I hope those precious packets of lichens have met with no mischance. You remember perhaps when they went to you. It was uncertain when you should send — but they were promised a place in the box, and doubtless had it, if it went. I do not myself remember about the time that the Exsiccata for the Museum went — but the other parcels were after that I think.

It is too bad for me to trouble you about this, which has probably long since been disposed of, & passed from your memory — but you will much help both me and Mr Lander if you can tell me anything: as he is locked up (he says till June) by the ice, and finds, as he writes, that his parcels have turned out, after all, to be a parcel for his great Bugbear Montagne — & nothing at all for him.

I am and have been hard at work on my Report on the N. Pacif. Lichens, and have made good progress. I shall

Amherst
2 May 1859

Dear Doctor

I was very glad, I assure you to receive yours of the 28th, with the very kind particular information about the little parcels of Lichens. I think with you that they must have come safe, and are probably in Mr. Sargent's hands, and shall write to Islander, & tell him to enquire as you suggest. The papers say that the past winter has been very mild on the North of Europe and that the gulf of Finland has not been wholly closed. But Dr. V. gives us an worse account,

Wrangel's account, as quoted by Dr. Lindley, Cuba, Carolina, Louisiana, Canada, Newfoundland, Labrador, Greenland, Iceland, Lapland, Siberia, Nova Zembla, Spitzbergen, Nova Zembla, Siberia, Lapland, Iceland, Greenland, Labrador, Newfoundland, Canada, Louisiana, Carolina, Cuba, Lindley, Dr. quoted as account's Wrangel's

& think the Swedish Academy has honored itself by choosing an American botanist — I can't say more. It is my impression that their number is extremely limited.

I sent my letter to the address in Cuba which Wright sent to me. I advance, equo pede, on the Enum[n] of N. Amer. Lichens, and the Report on the N. Pacific Expl. Exp[n]. Both are nearly half done, but the hardest part is to come. I shall do nothing else till they are finished.

I shall gladly ask Mr. Sprague to make the drawings, & will myself do the thing you said — let him do it as soon as you please, or find convenient

Yours truly
Edw. Tuckerman

and say, the ice will not
let him leave for Paris to
be got there before June!

His work has been standing
still during his separation but
I hope will go on again. M.
Montagu writes that he has
returned to his native wilder-
ness, from which he ought never
to have emerged.

I could not hear written
a notice of the writings of Dis-
raeli without such a revision
of all my knowledge as I am
never ready for. As it goes on
I find I cannot agree to all
that this new partisant for
fame proposes — and find much
that seems rash & inconsiderate.
So I am glad I have waited.
The great advantage of his works
is that they give a collected
what is known (nearly complete
it is hitherto) on the family —
We had nothing of the sort, and
the Beth can were too old to
undertake the laborious necessary
to see anything. We shall owe
it to D.I. that he has done
this, if he completes his work
& I think too, we shall owe him
special thanks for his labours in
several difficult lives.

But enough of this.

Amherst
20 Oct 1859

Dear Dadie

I beg you to accept a few of my Blue Peronaeae. The parcel went this afternoon, and will be at the Station of the Worcester R. Road to morrow. It has been kept waiting a week or more for the departure of your dealer in such things, and will perhaps need looking over. I am hoping soon to see the Carlton treasures. A note from W Wright, dated 12th Sept. informs me of his desire that a Collection should be put up

Dresser "Lectures on Botany in the South Kensington Museum" & the Author of several works mentioned by him, sends to our "Senate" as he styles it, a paper, aiming to shew that Sepals Petals & are metamorphoses not of the Carpels but the petiole of leaves — with various illustrations and proofs of this — and ends (in Conveyancer with a pretty strong complimentary allusion to Dr Hitchcock, as author of the "Religion of Geology") to be made a "Dr of Philosophy" — urging that his new views fail of recognition at home. It was referred to me — and so I venture to ask you if I am wrong in supposing that Mr D's views are less important than he thinks, and that the College should not grant his prayer? Believe me

Yours sincerely Edw Tuckerman

for sale before he collects any more, as he says he may possibly be collecting for several years. I shall tell him that I will willingly do this, & study the plants as soon as I get through the North Pacific Ex. Exp. Collections.

I hope to be in town in November when I will bring a few specimens to beg your opinion upon.

Did you notice that the genius has omitted the genus Cardamine in the "Catalogue of Plants of Bragg's Manual"? That is, I should say, he has left out four species.

Sartwell furnishes me some Rod specimens for my next Herbarium. He writes that Dr. Boott will bring out another large collection of plants of Cuba in the Spring; & what you are probably aware.

W. Nuttall was a kind friend of mine about 1840 — and I made a great many excursions in his company, in the neighborhood of Boston. I was much shocked to hear of his death — Perhaps he might have lived longer had he stayed in America?

Will you let me hear from you with a quotation. Asa Mr. Christopher

Amherst
13 June 1859.

Dear Doctor

W. Sprague shall be heartily welcome here — & I don't not the pleasant country around us will really set him up — and I wish you would send him — a sparer welcome — but if possible I should much desire that the work on the Lichens could be postponed till he is able to do it with the great Microscope. I have injured the best lens in my instrument, a French one supplied by Widdifield in place of Schiedt's glass — which I broke to

Yours otherwise to belong only to the Middle & South of Europe. (D. C. Peters.)

My lectures have not been begun yet, owing to the tardiness of Prof. Shepard in getting through his course of Mineralogy: but the Students are busy collecting their herbaria which, I find, and retain as a required and important part of the course.

These Herbaria make a fine show at the Exam" being often very really just up. I am inexperienced enough in teaching, and shall not feel at home for some years. Your works are now the text-book, — Wood having departed when I entered.

Yours Sincerely
Dr Tuckerman.

piece in my clumsy hand this — and he will not be able to do as good work as with the Cambridge instrument. But why can't he come here for his real, or what he ought to make his real object, health, and why may he not leave the work till another opportunity at home? I am quite sure the microscope will not suit him. I mean to order a new Lens from Schieck. Then I have no micrometer eye-piece that will give the required measurements in millimetres — those on the Lens

making for me this long time. But let me repeat that W. S. Shall be heartily welcome — I care with [illegible] to make him comfortable while he is with us. The weather is very bad but the country looks beautiful. Only String-beans in an unfair state — & a very few Sparrow-Grass effects to look with me.

I enclose an Alyssum which I suppose A. calycinum; it appeared this Spring in a piece of ground seeded with what is called "Lawn-grass" seed last fall. According to D.C. (Geog. Bot.) it is only since 1835 that that Sp. has made its appearance in Great Britain, introduced in the same way.

Amherst
31 Oct. 1859

Dear Doctor

I wish to thank you for your few but weighty words on to M^r Drespa & his application. I read his paper, and looked at some neat cut a specimen which accompanied it, and made up my mind that, probably, his discovery were less important than he supposed. — This you have fully confirmed. The slight modification of language, which some of them, which appear to be made out, will make necessary in Botanical works — can hardly be enough to prejudice any fair reputation that he may be entitled to — or deprive him of any honours that he may merit, at home.

limit, this position you may feel some interest in particular enquiries. Should I so, it will give me (need I say it?) no little pleasure to be informed of any point that you wish investigated, that I may, if able, try to explain it.

A. G.

The box arrived safely, with its very valuable contents. I have only glanced at a single parcel of the Lichens, and cannot at present. Mr Spruce's drawings are exquisite. I am making such references as is necessary, to them, in my manuscript, and shall then, as you suggest, send them, & the Mss. to your kind hands, and as soon as possible.

I am sorry about the Apples. I sent some to my Aunt in Boston, and they also failed in getting them. But tonight, they with that they have come — and that the trouble was caused by the RR officials who carelessly "omitted checking them off." I wish I could have prevented it — but fear they will cost you more than they ought to. There has been unofficially about paying them, and so I have omitted it of late, but will do so the next time I ask you to taste my Apples: that you may escape (as of course Lawlessness & Wildness) so much South.

Yours sincerely
Asa Gray

P. S. (Errors of Press noted by E. L.)

Manual p. 505, l. 23, for 9 read 10.
" Cercis left out in Index.
" p. 664, l. 18, for bogs read logs.
" p. 658, for Leskea read Aestiveea.
" p. 565. T. ovina. Author omitted.
" p. 573. "lots," a local expr.n? Excuse this if hypercritical.

In noting an omission in the Catalogue, in my last, I omitted to say how convenient it is. I think a little of botanizing about Norfolk, & in Sussex, Va. next spring. As this is your Southern

Amherst
31 Jan. 1860

Dear Doctor

I received tonight the proof and have just finished correcting it. Floerke spelled his own name as I have done, and so I prefer it: and the only other allowable form, as I suppose, is Flörke. So I have not followed your correction to Flœrke (as I understand it at least) One of his publications was "Deutsche Lichenen" which is commonly cited "D. Lich."

I only wanted to trouble you with a sort of request to procure for me the money promised for my expenses, because I did not know when to apply to — and also because they told me "vouchers" would be required of my performance of the revision; to which I replied that I could only ask you to attest to it.

But I must ask you to let me when you write again if I ought not to present Mr. Sprague's bill at the same time with my own; and I wish also to request a very few lines from you stating that I have done the labour of revision agreed to, and am entitled to the pay therefor agreed upon. Will this, (in any terms that you please) be agreeable to you — if not, I beg you will not think of it.

The Printed matter looks very well — Thanks to your kind Supervision; for which I feel much indebted in the correction of the proofs.

I will await anxiously to hear from you as to the Extra Copies. I am glad to hear that I am to have one anxiously from "the Committee"

Sincerely yours

Lott. Tuckerman

Amherst
3 March 1860

Dear Doctor

I have sometime since returned the last proof of the Lectures, and have now the Review by me, having also made an Index, like M. Sullivant's. But observing several mistakes in the Review (one is Montague for Montague. which I remember correcting) I at once wrote to the Printers to know if they could still correct the typos — & have not yet heard from them. As to the sheet sent in your letter, I was struck myself with the defects of the original — but knew not how to copy it — and shall I weary your patience too much, if I now return it to you, to do just what you please with it? Perhaps it will give you but a moment's trouble.

But I wish also to thank you for sending me, in advance of publication, the statement of the way in which you regard the new book of M. Darwin. I have not seen it, and may not, immediately — perhaps not till the Summer, when I do all important reading. It seems to me, now, to belong to transcendental science, though an Englishman cannot treat even that in any other way than his own materialistic one; at least, I suppose, not easily. But I ought not to say a single word: — for the author was fully competent to his work — and that should be studied before any opinion is expressed.

So far as my own studies have gone, all I can say is — that I find myself tending to see fewer types than once (that is in Lichens) but when one enters that way of thought — the prospect is so bewildering as to be pleasant.

In an article I have ready for Atkinson's Journal, I have applied this to one or two groups of species of Physcia, which I now view as but varieties of two species. The discussion includes some seven or eight species named by Europeans on American specimens.

That is as far as I have any connexion with the discussion. I had rather keep the old species however, & will not give up one that I can help doing, nor do I expect to get beyond this humble thing, to any great interest in the general speculation of naturalists as to Origin of Species.

Thank you also for the notice from Mr. Stuart. My means is limited this year, as to the no of copies I can take of the extra ones.

Yours sincerely,

Edw. Tuckerman.

Amherst
7 March 1860

Dear Doctor

Shall I trouble you again? It seems to be inevitable. I prepared an article for Silliman's Journal for May; and wrote in Feb' to ask if they could take it. Not hearing, I wrote again. Since I have had an note, saying that the ms was full, but that they would be glad of my paper, and would do what they could for it. To-night arrived another, in reply to my second, saying that "the doubt xx is now removed, and converted into a certainty adverse to your wishes".

The terms was not pleasant, and I cannot account for them. But the writers (Silliman & Dana) go on to say that you "will print as much Lichen matter as you may choose to furnish, for the use in Proceedings of Amer. Academy."

The fact is that I should like to publish my Revision of the genus Physcia in North America, before Dr Nylander gets to that genus:— having drawn it up however, not by any means for that object, but because I had reached the genus in my new Enum" of North American Lichens: which I am now at work on, in connexion with the Cuba Lichens:— the N. Pacif. Report being all done except some parts that Nylander (who has the material, which I have not) is to do for me, as far as I can send bits of the specimens by letter.

However if you will print this paper, I shall gladly send it to you. It is about ready — needing only a little revision that will take perhaps half a day. Had I known when I could look to be able to publish in the Proceedings I should have postponed it from the first. But I do not suppose that the Editors of S. I. mean to say they had rather not receive papers from me? The letter from me was meant to be proper — and the second newly added to the first that "I should be very glad if they could take the paper"

Excuse this from Yours sincerely

Wm. Tuckerman

P.S. I have corrected the Revise of the Lichens of Wright's Exp". Found some 30 or more errors. Mostly my own; but not all. I also corrected the term "spore-cases" to "spore-sacks" which I like much better. The first was an impromptu — and I am glad to change it. I asked them to send me a Revise of the whole, after your changes had been made — with the proofs of the Index — and hope now the thing will be correct. The printers had not been careful in correcting your & my proofs!

Northwich
12 March 1850
5 P. M.

Dear Doctor

Your welcome letter has just come. I thank you very heartily for it. I had anticipated, myself, the reason you gave for Sillimans not welcoming the proposed paper — and think it sufficient. If they will only increase their Geographical department, and you will continue your department, I shall be well content; — and am sorry, that their Subscription list is no better.

I hasten to close, because I must get my parcel into the hands of the Engineman tonight. As to the Report

on the back of U. S. Expl. Exp.,
I had rather wait till I finish it,
& inserting what Sylander has
agreed to furnish me.

I hope the accompanying ms will
give you no trouble (as superintending
the printing) Part of it was
drawn up in one way, & part diff-
erently — but they will easily print
it uniformly — and it will be pretty
clear work I think.

I should like 25 copies of it.

Yours sincerely

Edw. Tuckerman

P.S. As to the Wilkes Report I am
only limited by my now limited purse.

Amherst
19 March 1860

Dear Doctor

Thank you for a note just received, and for the news that my ms has reached the printer. Not supposing that it wd leave you so soon, I have prepared another page, which I hope may be admitted. There will be no more.

My plan was to publish the Lichens of Wright as a Hist. Exsic., with regular tickets: to be done so soon as I shd be through studying them. It will not do, will it?, to send them abroad without names — but with it will not. I have made good progress, and done as much as could be expected in the time; & am doing nothing else. The preliminary study of my whole N. Amer. collection, only prepares me, and this often a necessary preparation, for taking up the West Indians.

The price is $5 for each volume; the first being represented by specimens labelled, placed in sheet of paper. If this does not diminish the desire to possess the work, I will send it, on your kindly informing me further.

Yours Sincerely
Dr. Tuckerman

I told Mr. Wright, when I took his
N. Pacif. coll^n that if he was in a hurry,
he had better send them to Paris; — that
nowhere else, or only at Kew, were
the collections extensive enough, & well
enough named — to warrant a speedy
determination — and he assented to
my doing the best with his plants, which
can be done, with such a herbarium
as I possess. He is one of the best I
believe: but still feeble in the difficult
genera; as were the collections which I
was most familiar with, — I leave
the tropical difficulties now: and I
am often puzzled, where one would not
be at Paris. But I wish to com-
plete the "Cuban Letters", as I have
begun them, and at the same time
to satisfy Mr. Wright, as to the distri-
bution of the specimens.

If he leaves them with me, till I get
through, it will take me the whole of
the year now begun, to finish; when the
mss., the tickets, and the sets may all
be ready. Nor do I know how to
do any differently. I have not written
to him lately because I desired to be
able to tell him that his N. Pacif.
work was done, which it is not quite
as Dr A. is to do part. If he
leaves me, Dr. A. will have to
do more, though of course I could
decline to send them, the Cuba coll^n away —

On the whole I do not see why he
should wish me to despatch them
any faster than I am doing, which
is as fast as I can!

Shall have much pleasure in
sending my Exsiccata, but the first
volume is no longer to be had bound.

Amherst
23 March 1860

Dear Doctor

I have just received your note about the Index. I am sorry that I got no nearer right, but my only model was a sheet of Mr. Sullivan's Index without the explanatory notice prefixed to the Index now sent; — and there was nothing in it to show that Synonyms were included. It will also be impossible for me to do anything without a return of the revise — for I sent the whole back (I thought for good and all) some days since.

Perhaps I had best wait till you advise me (I do assure you that I am heartily sorry to be compelled to trouble you so) but you will make me!

I wrote in reply to Your note announcing the new parcel for Wright and enclosed another page of my paper in the Proceedings: and again acknowledging the parcel — and re-stating my notions about the distribution of Wright's Lichens. Now I mean to take the best set, after his own, (besides my own specimens, coming to me as elaborator) for the Berlin Herbarium — and others will doubtless go abroad. And if they so remain, I do not see why any Lichenist who is publishing may not, with perfect right, describe what he finds new. So I have concluded that nothing can be done with the plants till I can work them up. Mr. Wright is particularly full in the obscurer genera — some that nobody knows much about — e.g. Nylander. I am sure you must see that I ought to be hurried in working up such things. If convenient (which it is not) I should rather advance Mr. Wright's to satisfy him, and secure the proper work up of his discoveries; which it is his very credit to have made in the obscurest parts of his field.

But enough of this for—

Yours sincerely

[illegible]man

Amherst
21 March 60

Dear Doctor

I write only to say that the large parcel Kirby announced by Fox reached me today. Let me add to what I said in my last, that it only adds to the difficulty of any immediate publn — being almost altogether made up of Tropical crustacean lichens.

Australian girls now know more than any one of these. Montagne has been studying them 20 years, but has seen much less N., who has seen everything. I said almost so — that could be seen in the herbaria. I have not entered this field of study — & my herbm is poorly fitted for it. Still I

am not at all afraid of the work — Ent must take time. I have now got those about everything at the Crustacean families — but till I begin those, cannot say how long they may take me. I shd be glad to take them to Paris — or perhaps Berlin — but cannot. So Mr. Wright will have to have patience; especially as I am position "over head and ears" in the study of his plants, and as anxious to be through as he can be to have me.

My plan is, as already suggested, to publish them as "Lichens Cubani", with proper tickets. It will be a splendid set, and do him the greatest credit. But its peculiar value is in the crustaceous lichens — and then all require the microscope; and can hardly be named with any safety without comparison with authenticated specs; or where necessary despatch of letter to those who have them.

Now is my statement. Your opinion will of course be of the highest value with

Yours sincerely

Edw. Tuckerman

Amherst
23 Dec. 1861

Dear Doctor

The Lichens have safely reached this Storehouse of Cuba Lichens of which I am custodian. They will require some time to be digested, and the larger part belong to Opegraphaceæ & Verrucariaceæ, the two tribes not yet reached in my investigations (the former will be begun the present winter) I do not doubt there is much novelty & value. Indeed there is no reason to suppose that any part of the tropics (unless it be Guiana, where the French have made large collections of which I am quite ignorant) have been examined more thoroughly, and I believe that Mr. Wright's Lichens will bring his great

very often the far more difficult task of deciding what my species is — or how I shall decide to arrange my species. This costs a great deal of labour.

Tony Fries has published two more-lesser works in the Upsal Universities — one on the Lichens of the Artic North (embracing Dr Kahl's Greenland collection) and another on the European Genera. He follows the Sporological School of Massalongo, of — avoiding some excesses or subdivision of others of that School; & spite of his extreme bitterness towards Nylander, adopting a good deal of his work. But I suspect he has not his father's genius.

Excuse my prolixity

Yours Sincerely

E. Tuckerman

credit, whatsoever this paper for them to be published.

You may depend nobody desires the work to be completed as much as I do — It oppresses me sometimes — so slow & unsatisfactory in my progress — where uncertainty existed at the best previously more than anything else — but I shall try to get through it.

The constant use of the Microscope for the past year has tired my strong eyes — & I have now, when I read them much, to be careful.

Then add to all this that the Systema is all in flux. Fries's Reformatio was all we wanted once. But now spores & spermatia are at the bottom of all the proposed Methods — not one of which appears to me to be of a, fair state, commanding merit.

Nylander's recent, leaving the old Systema more undisturbed than most of the other methods, while at the same time it takes account more or less of the spores pleased me at once, but it is largely based on the spermatia; an obscure, difficult & often impossible criterion; so that I shall hardly be able to adopt the system in my general North American Enumeration. As the tendency in the purely sporological systems, recently seem sometimes to stand for species, species for genus, & genus for tribe: at least that is the tendency. But the spores are a better because easier and more practical criterion than the spermatia.

Thus in addition to the perplexities in determining what my species are, I have

Amherst
17 Jan. 1862

Dear Doctr

I receive your last note with its contents in good condition, and return specimens of each of the plant, secured to the original tickets.

400. Evernia vulpina, ach. — published in my Lys. n. 53, from Fremont's coll^n. A characteristic Western species, the no. eastern loc^y known to me being the "Black Hills on the Upper Missouri". Maryland W. to the coast — but not rare in Europe.

399. Cetraria juniperina, ach. a much dwarfed state of the var. terrestris, Schær. I never saw anything quite like it: the more developed form occurs in Arctic America, pretty much as in Europe.

398. Dactylina madreporiformis, Tuckerm. This is the old Dufourea madreporiformis, known since Jacquin's time as an inhab^{t} of the European alps, but never found fertile. Curiously, this is in the same state. The genus Dufourea was made up of ~~coal~~ plants looking a good deal alike — the most of them however not known then in fruit — & contains, as now understood, at least four generic types. One of these the Duf. arctica, Hook., Nylander first separated as a new type, the fructification being known. Duf. ramulosa, Hook., like the last, American, & unknown to Nyl^{r}, but fertile spec^{s} obt^{d} by Wright at Behring's Strait, enable me to ups it without doubt to Dactylina. And this last sufficiently bridges the interval between D. arctica, and Dufourea madreporiformis, to make it, in my judgment, most natural to add the latter to Dactylina. I will put all this in^{o} extenso in my next pub^{n} Obs. on Lich., in the Proceedg^{s} of the Acad^{y}. D. madreporiformis is wholly new to America. But I possess a fragment appar^{y} belong^{g} to it (from the Hooker^{n} herb., but evidently mark^{d} to Nyl^{r}) which was for N. Holland.

Wright's last parcel of course interrupts my continuous work on his main collection, till I can get all the new matter in to the places it belongs in, up to where I was in the main coll^{n}. No little revision of previous work is thus occasioned, with benefit I hope, but occasions delay.

Excuse my long lichenological harangue & believe me thankfully and sincerely yours

E. Tuckerman

Prof. Gray.

Cambridge 17th Dec. 1862.

Dear Wright, Here is Tuckerman's last. I shall communicate with him to-day. It is far better than any I have ever read from Dr. W. Curtis up to the last sheet but one and that of only 4 pages. I shall soon send it to you — with whatever else there may be.

I hope Tuckerman will now really take hold of the Lichens, and have them get to market.

170 Tremont House
11 Dec. 1862

Dear Doctor

From your last note, enclosing two lichens of the alpine reg.n of the Rocky mtns, I infer that Mr. Wright has removed fm. his former residence.

May I then trouble you to give me his present address? It has been my desire & expectation to call upon you, but I cannot well leave my work in the mornings, & I have also suffered from obstinate colds.

I propose to put up everything in order for sale, except the two last (& most obscure) tribes, Lecanora with the epiphylline species, & a parcel of doubtful things — in spirit. The remaining things, above ~~reported~~ excepted, I shall go on working on through the winter, and shall know, by & by, whether Mr. Wright is pleased with my plan of making Exsiccati, & whether he has any on hand in Cuba, the basis of a 2nd Emission; — or whether he prefers that

I sh^d engage Dr Nylander to work up the whole remainder. In the latter case, the requisite preliminary study will then be done, & I shall be able to submit Specimens of everything to the Scandinavian Professor without loss of time. You are aware that this cd not be done without a serious study of the whole collection. In the obscure tribes before me, such study, leading only to grouping of what appear, prima facie, to be distinct existences, is very slow and tedious work, — much more so than the microscopical observations, which are to complete the investigations. And you will admit at once that when the same thing (to appearance) is presented to you in a dozen specimens or one bunch, a dozen slightly different or another & so on, & that every specimen has to undergo searching with your Coadjutor's lens — that known as infinite. The genus I am now on (Parmelia) is especially obscure — so much so that Fries rejected it, as exactly matching conditions of three Genera: but the species contradict this view, & compel us once more to its study.

I propose to put the specimens up in the way that Lindig's & Granville Lichens were put up at the Paris Museum: square pockets of neat paper, with a loose ticket within, & another affixed to each pocket on the outside. This is neat & cheap.

The Rocky Mt^n Lichens are always very interesting to me, when they come from the alpine parts.

Cetraria nivalis (of my Exsicc.,) has only previously been found by Dr Hayden. It is one of the characteristic lichens of the alpine districts of the North^n hemisphere. The other, C. juniperina, in the same variety, is also alpine, & was sent by you last year. But this is now the key-release.

I shall greatly desire to see the Rock Specimens.

Yours sincerely

E. Tuckerman

170 Mount Horeb
17 Dec. 1861

Dear Doctor

In your last note you alluded to Mr. Wright's present residence in a way which led me to doubt whether I had his proper address — & I wrote to you as soon as I had been able to make up my mind, as to what I wished to say to him, begging for his address.

The letter was posted last week, but I fear it has not reached you, especially as I neglected to add "College" to the address.

Will you kindly tell me, that I may at once communicate with Mr. W.? I am very desirous of hearing & sending him — & quite as much so, of setting straight with his plants. And in the letter above referred to I stated that my plan is, to put up

the whole collection, as soon as I return home, in April — except the two last tribes, which, exist the parcel of epiphyllum forms, & another of wholly doubtful ones, will, I fancy make a good base for a second emission, with what M. W. has now in hand.

And in case M. W. does not approve this, I shall have the cells all ready, to furnish Dr Sylander with samples of everything for his determination — which however will take time, though doubtless less than if I had to determine them.

Yours sincerely
E. Lindman

Professor Gray.

P.S. The work of this winter will go a good way towards determining the tribes I have in hand, & as I have said, whether determined or not, the specimens will be quite ready for communication to the European botanists.

E. L.

Amherst
16 May 1863

Dear Doctor

Herewith I beg to submit to you the paper which I propose to use for the Lichens of Cuba.

I reckon of the specimens proposed to be now issued 20 sets of about 140 nos; & about 30 ~~nos~~ {numbers species over, of which there are only from 2 to 10 specimens each.

A ream of paper of the size sent will more than serve therefore, and on the appearance of it suits you, I shall send for it at once.

I have affixed to the outside the kind of ticket I incline to adopt, and send also one of the capsules of the Lindig coll^n to compare.

There will need to be about 6200 copies of the ticket printed - if a loose one is put inside with the specimen - or 3100, if the outside ticket fastened to the capsule is enough.

Will you kindly tell me about what the tickets will cost, on white paper? I wish to save Mr Wright the expense of issuing the collection, but do not desire to spend any more than shall be required to give the collection a good appearance. I enclose also a more expensive paper but do not know that anything will be gained by adopting it, and it is not near as strong, as the Manilla, of course.

The specimens may be put up in sheets of the same paper, as you suggested, after being secured in their capsules.

Thank you for the very interesting & valuable paper from Lithuania, but I wished twice as long.

Yours Sincerely
W. Lauderman

P.S.

If no paper suitable & cheap enough can be found, I shall expect to be able to send you the sets for distribution, in a very short time.
I am expecting Mr Wrights Cuban collections, which he informed me in his last letter, would be forwarded. These, with the two tribes & miscellaneous specimens kept back from the 1st will make up the second part of the work, to be issued hereafter.

Amherst
20 May 1863

Dear Doctor

Thank you for your reply.

What led me to undertake an immediate issue of the Cuba Lichens was your suggestion that it might be done by nos, referring to a future publication of the species. I did not intend to attach the names to the tickets. Leidig's large coll^n is still in the same state, Nylander having only published a few new things. I see no objection to this way, myself: & thought I could with entire satisfaction to myself, now publish the names of 2/3 of the plants to be issued, — the other third it would be risking more or less to venture — the new species, of which a large pack consists, not having been criticised, as they

To explain why I venture to trouble you with this, let me say that I once ordered some papers of a horse in Boston, & they came, all cut in halves — so that I had to get people here to gum them together again! I have no knowledge of the merchant, but he keeps a little way down Water St.

Yours Sincerely
Edw. Tuckerman

P.S.
I shall have another paper for the Proceedings of the Academy in the course of this year. As long as I am a member I expect to continue to offer the Academy my observations & have reason to think they will be acceptable. I do not propose to offer any to the Nat. Hist. Society, and indeed I have for some time ceased to be a member of that Association, which for some years kindly received my early publications, but was never properly a medium for botanists.

ought to be, before publication, in Europe. How can I tell when Dr Hyland will get over the offence my last paper gave him. It is enough that he had no good reason in the paper, & I solemnly say that he has no other, — but the fact is that he has wholly discontinued his correspondence, since the angry appeal ag^st my unfortunate memoir, & left the various business matters between us, with a large balance against him, wholly without settlement. So I cannot tell when I may once more have the benefit of his knowledge; (unconscious of any reason to the contrary) I look for a letter by every mail.

In this state of things there is a good deal which I shall not care to risk the publishing of. But when the work of putting up the specimens is in hand, I may decide to name all, or at least the new things; writing, which I prefer to printing.

May I ask you to order the tickets for me, according to the specimen sent, & the no. given in my last?

And will you, to prevent the possibility of mistake, kindly help me so far as to look at, & take for me the ream of paper of the quality which you approved, at I sent, and R. C. Jones's, Water St. Boston, to be sent, with bill, collect on delivery, by Express, to me at Amh. Mass.

33 Chauncy St.
Boston
29 Dec. 1863.

Dear Doctor

I believe you made some enquiries about the Lichens of the U. S. Pacific Expedition, with a view to their illustration by plates — last year, or early in this: and that I then said I did not think, that (considering the new species only) there was material capable of being very advantageously used in the way spoken of.

The new species were mostly such as are by no means easy to make intelligible by plates: — some of them having, for instance, the external appearance of a variety of thuja, and depending entirely on their spores (just as well characterizable by description as by figure) for their systematic position; and others being small, & wretchedly incomplete.

There are two or three fine foliaceous Lichens indeed, as Sticta Wrightii, and

Parmelia Japonica &c., which are well worthy of thorough illustration, and if other than new species may be chosen for illustration I have no doubt that I can add one.

But all is at present, and I hope therefore the time allotted for the work will permit me to postpone the procuring of the materials till Spring.

If you decide on the illustration of the species mentioned above, as well as of such others as I may think most desirable, within, of course, the liberal limit allowed you name — I shall very gladly prepare the specimens for Mr. Sprague: and I think, could I once get through the mechanical drudgery of the wearying study of Cuba Lichens, might probably feel the interest I ought to, in so rare an opportunity to illustrate plants in which I feel so strong an interest, — but when must the specimens be delivered to the draughtsman? Will it possibly do in the Spring?

Yours sincerely.

Sw. Tuckerman

P.S. I have been thinking of going to Cambridge in pursuance of your kind invitation, but vetat Wright, vetant Lichenes! The first pause I can make however, I shall turn my steps toward the Botanic Garden.

May I expect an opportunity during the winter, or in early spring, to publish a continuation of my papers in the Proceedings? If you prefer, the English descriptions shall be abridged, or even omitted; — my object being to get a quantity of new species (some 30 or 40) into print.

E. T.

I understand to constitute a single species from Cuba, Lecidea microphyllina, T. The lichen has a pretty squamulose thallus in my a, & varies a little in the colour & division of the squamules — but passes in my β into granulose conditions. But Nylander says a, makes three new species
L. microphyllina
L. tryptophyllina
L. leucophyllina — and β three more. The spores in all these forms are acicular & colourless; & vary in no respect whatever, as I look at them: But if they are, constantly, (which I do not allow) a little longer in one form than in another, the shape being the same; & if they do act a little differently (that is the hymenium) with iodine — who is to pronounce that, other things being equal, these two are to settle the question whether the forms are "species" or "varieties"? There are other cases of the like sort, where I cannot accept the new species. I am sorry to have let my pen run on in this way. But it may shew you why it is difficult to do without knowing what Dr Nylander has done; in any sense which he has had much to do with, & how difficult sometimes it is to manage the information he gives us.

I beg to offer Mrs Gray for my wife & myself the best wishes of the season,

Accept of the same from yours sincerely

E. Tuckerman

Boston

8 Jan. 1864

Dear Doctor

I shall go to Amherst within the month, & get all the materials for Mr. Sprague. I am sorry that you are to lose him.

I cannot get ready to make up my paper for the Proceedings till the Lichens of Cuba are off my hands, which will not be before Spring.

I have just heard again from Nylander. He writes that he has no hope of seeing his Synopsis. His knowledge of what is described is so much greater than any one else's, that it is a great pity that he cannot be encouraged to put it into print.

I have proposed to him to publish researches (in some form that will not cost him anything) of the remaining genera, as he understands them, and

hope he will at least be able to do that. But he says he has to do other work for a living. His professorship is also some (at Helsingfors) whatever it may have been worth to him. If I had a thousand dollars to spare I would see that his publisher invited him to continue.

Just think what a state of uncertainty his failure leaves us in! In the first place he alone knows the Acharian collection (at Helsingfors) thoroughly. He alone has been through the tropical collections upon which Fée founded his work in 1824. He has revised the whole Hookerian collection, and knows also that of the Paris Museum. And his correspondence with me has put him in possession of all my collections, so far as he has reached the genera, & so far as he has gone. Is this amount of real knowledge, already embraced in one head, which, I think, should not be lost, whether the head belong to a Fin, and be otherwise an agreeable one, or not. For myself I distrust his scientific judgment, and dislike his hurrying to a conclusion, & putting it in print, & then, either taking it wholly back, or qualifying it very much, the very next time he writes. Add to this his want of courtesy & consideration of others.

When he thinks he has a new species, he seems to take it by the collar, turn it round with a hasty glance, & then, sticking its name on it, kick it (no other word suits me) out with a postscript that it probably, after all is not a new species. He does this again & again. You have no elaborate description of this plant, which is named as new, and yet is "probably not new." You have not always a sufficient character. There are instances (as on the last page of the Synopsis) where a lichen, as Physcia confluens, L.c. has no single character assigned to distinguish it from the species going before it, except perhaps a slight difference in the measurements of the spores. Such a difference may indeed indicate a species, but if the systematist can find no other difference, I doubt the species.

He is getting now to lay a great stress on this minute measurement of spores, taken together with the behaviour of the hymenium with iodine. And I will shew you what this leads to.

I sent him a fine suite of forms which

33 Charles St.
Boston
16 Feb 1864

I return the note of Dr Hooker and thank you for reading it to me. Had I thought, when I filled an vacant hour with the note to you upon the Feinish raven — that you would think as well of it as you did, I should have taken a little more time and satisfied, myself at least, a little better. I am not surprised nor disappointed at Dr Hooker's decision. Nor can I look at the scientific merit that the raven in question now brings as any real advantage to science. His papers look well, and show a master of labour, and it is the labour of a man who ought to be more learned in what he writes on than any body else. But the newer species (too many of them) are abandoned by the author himself as soon as he has

in relation to the specimens, & an acknowledgment also of a handsome sum of money I had sent him (nominally to pay for postages & for his various publications sent by mail, but in fact for exceeding, as meant to, any such outlays) let me know it, before I published the Species. Instead of that, in a short time comes a proof-sheet of a new memoir of his in the Annales, in which my ms. determinations are made to figure in connexion with his own & also criticised to such an extent, that one of my Species is expanded into six, & another old species, as I understand it, into five, making eleven in all. This surprised me, but I knew his way, & that I had not guarded against this use of the specimens — & so, in corresponding, was very careful not to say much (writing the letter twice in short) & expressly to add that I had no right to blame him though it would have been more agreeable had he waited till I had published the Species.

His reply is exceedingly harsh, as if I had injured him beyond forgiveness: but as he added that he had "tribulations assez grandes pour que des nouvelles ne peuvent m'être qu'excessivement décourageantes" I felt sure he must have trials, & have tried to make him feel better; even telling him of your kind interest & willingness to attract the attention of European friends to the importance of his work, in

finished his brief description; or at least as soon as he arrives at a chance to write them, in a new memoir; while old species suffer in a like way from various sorts of intermeddling with them, and changes upon changes of the Nylander-dean idea. His first book was the résumé of the French Lichens, & was an able criticism of the Lichen-Flora of Middle & Western Europe — marked indeed with his present defects of manner as regards the opinions of other writers — and a rather extravagant and hasty reduction or destruction of what are commonly taken for species (as at p. 109 where 10 species of Koerber's Syst., are, in a note, and without discussion pronounced "names" of one species, and at p. 116, where 5 more with an "etc.," &c, in like manner, "denominations" of another — a good part of which 15 curious to see, are now recognized, and not a few really doubtful are added to them by the author himself) but, as a whole, I think, an interesting and instructive application of modern views to the old system, which he appears to seek, not as the sporologists to destroy, but only to reform. It was this tendency as I regarded it (it is important to say that the author rejoiced this credit, & claimed that of a founder of a new system, based on the Spermagones & their contents, a way in which I never accepted his work) which made me a willing learner of all he would teach me: so that, at last, he has come (in his Bot. Jeibung article, taking me for differing from him) to claim me as a follower, which I never was, any more than of others.

But excuse this, which an awkward cold makes, I fear, unpardonably dull. Let me get add one more little narrative. Having occasion, last autumn, to get the remainder of the determinations of Graphidei of the U. Pacif. Exped., which Mr. H. had agreed to make — I sent a small packet to him & included in it my new Lecidea of Cuba: with the hope that if I had made mistakes, he would,

Amherst
28 May 1864

Dear Doctor

Being at length here again, I write to inform you, that you may kindly direct the proof-sheets of my paper to be sent to me.

I wish to have ~~the~~ a <u>final</u> proof of the matter as set up, as well as the <u>earliest</u> one, so that I may, if possible, prevent the serious mistakes which occurred in the last portion printed, where, owing to a change in the pointing, one diagnosis was rendered almost absurd.

If "Pallisot's Expedition" was very thorough in hunting up lichens, it is

I Suppose, possible, that the two Species from the Rocky mtns may have been found, in which case, if Nylander has the naming of the Collection, he may anticipate my names — but otherwise I am in no anxiety about the printing.

Yours Sincerely

Edw. Lechmere

Amherst
8 June 1864

Dear Doctor

I am constrained to ask of your kindness an insertion of the accompanying pages in their places, in my memoir. Such curious additions to the N. American Flora should be added to our own record, and not left to foreign illustration; and all may well read the Hookerian herbarium, whenever a botanist, in British interest may take the trouble to collect them.

And I include also a pleasing photograph of Doris, and one also of his son, just sent to me in a letter acknowledging the forwarding of the Lichenes Cubae. I must ask you to return them, but shall be happy, if you are not supplied, to procure another

of the venerable successor of Lunidus.

For myself I hope hereafter to get one more suitable for framing, which is the only way I can generally manage portraits, not having undertaken as yet to make a book of covers.

I wish also to thank you for a copy of the published fragment of the Lichenographia Britannica, & should be glad to know if I am indebted for it, as appears by the cover, to any English source. I have long possessed the book, which had a Supplement added to it by Mr. Turner, included in my copy, and looked upon it, incomplete as it is, and unluckily devoted mainly to imperfect and abnormal conditions of Lichenose vegetation, with which the author intended only to begin — with a peculiar reverence.

I will therefore send it to Mr. Russell, if you do not reclaim it.

I shall hope now that the Printers may soon take up the work on the "Proceedings."

Sincerely yours

Edw. Tuckerman

Amherst
11 June 1864

Dear Doctor

Thank you for your note. It gave me no little pleasure to find you so much pleased with the picture of Price. I shall not forget to procure another, and shall write as soon as I hear again, which will be when my packet is received in Upsal.

I mean to begin a collection myself, having already two or three, sent me lately by southern correspondents, and shall beg one of you, when I next come to Cambridge.

As to Bolanderi, let me put down, somewhat at length, why I prefer it, with all the reasons. The name is either Greek or Latin, or it is German of the Swedish type. In the former case it is one of a set of words compounded with ἀνήρ (becoming ἀνδρός, and in Latin ander) in which undoubtedly the final e of the nominative is lost in the genitive ι; & we have Alexandri and Alexandro. And there are modern

But you invited this, and so I will not apologize.

Will you make my acknowledgments to Sir Wm. Hooker for his kind gift of the Lich. Brit., none the less valued by me, that I had the book — when you write again? I shall never forget the great kindness of Sir William and Lady Hooker, and I often recall the visit I made to Kew. And it was to him that I owed my visit to Mr Borrer.

Sincerely yours

Asa Gray

P. S. I sent by Express, because I made no copy of my MS, and am always a little unwilling to trust papers of value to country politicians who may happen to be, for the time, Postmasters. They do not always know enough to handle letters safely, as I have found here in times now happily past.

compounds of this sort, as Neander (New-man) what to form Neandri & Neandra. I had sometimes suspected that Dryander might be another word of this class, derivable from δρῦς and ἀνήρ — but have no authority for it. Still notwithstanding such exceptions, the presumption must be that modern surnames of this form are not Greek-Latin — but, in the cases we have before us (Solander, Bolander &c) teutonic. In that case we have no longer to consider compounds with ἀνήρ or ander, but only the old German ending er, equivalent to man (as in Bauer and Baumann, Reiter and Reitmann, Miller and Milman, Parker and Parkman, &c) and the names we are especially considering are to be compared with the German England-er, and Holland-er, or our own New England-er. Sweden is full of territorial designations ending in land (as Oland, Smoland, Halland, Upland, Gotland &c) and when the Swedes took possession of the wild countries inhabited by the Esths & Finns, they put their own names on these countries as Esthland and Finland, and going a little further East of the last-named tract, they called it Ny (or New) land, an inhabitant of which would be a Nylander.

Now in these cases, the ending er has the same value and signification in Nylander that it has in New-Englander, or Parker, and we have no more right that I see to say Nylandri & Nylandra (or if Solander & Bolander be also, as is presumable, teutonic, to say Solandri & Solandra) than to say Parkri or Parkrii, or Parkra. And I fancy an etymologist might dilate on the very essential character of the termination er, and that it should certainly be emasculated in such words as Solandra.

And I confess that while Nyland (New-land) and Nylander (Newlander) are quite intelligible teutonic words, and appeal at once to him whose tongue is teutonic, the former a country, & the latter one of its inhabitants — Nylandra is less clear, (compare New Englandra) and the notion of personality involved in the er is at least very dim; as it would be in Parkria, as compared with Parkeria, which at once presents to us something connected with a Parker — that is a man having a certain relation to a park, — or a surname originating in such a name of occupation, while the former is obscure.

Amherst
20 Feb. 1865

Dear Doctor

I thank you very much for remembering me in the Bolivian lichens. I am ashamed dark Nylander's paper on them, which reckons some species of no small interest.

If you will kindly send them, & a little memorandum which I shall receive for as representing 16 sp., I will do so with gladness & alacrity.

It has proved impossible for me to go on any further with the study of species, until I have settled & put into print my views of genera, generally, & of system — work in by no means definitely settled in Lichens. — I call the memoir,

"The Insects of N. American Lichens"
is nearly half-done, will
make some 200 or more octavo
pages. As I am very desirous
to get it out before Fries is taken
away, & am also uncertain when
I can get it published (if accep-
ted by the Academy), I have
made some enquiries with a view
to printing it myself.

But if it is quite acceptable
(so far as anything can be, in esse)
& will be favoured with an
early publication by the Academy,
I shall be glad to save the
expence of printing.

It will be done within this
present season, that is within
this year. Now I desire
to take the best course with
it, & shall be grateful for
your kind advice. Whether
it appear as a Memoir or not
is indiff^t to me. If I print
myself, I shall do it in the
cheapest form & way.

Excuse when you read the
Lichens with obliging

Dear Sirs

E. Tuckerman

P.S.

I cannot omit desiring
my kind remembrance to
Mr Wright, if he is with
you.

Amherst
1 March 1865

Dear Doctor

I thank you very much for the 2 shells. The specimens are excellent, & not one that is not desirable, one or two especially so. At 40^{fr} the century, these ought to be 8^{fr} I suppose, and 8^{fr} at 47^{c} the franc ought to be $3.76. Have I made any mistake? There are 20 n^{os}. From your kind note I should fancy I was mistaken in the above, but cannot read the figures differently.

It would be a great advantage to me if the Academy would undertake to finish my General Lock N. Amer., for one cannot

expect to be remunerated by the sale of such a work, and I have no right to be extravagant. I thank you much for the intimation that some such arrangement may be feasible.

I should gladly have joined the circle at Mr. Emerson's had it been in my power.

Mr. Leighton's publications are worth getting together I am sure, and the Eyricals contain many interesting pieces. I cannot guess why he has withdrawn his interest in me, but doubtless he supposes he has some reason.

Mr. Lowell has done nobly. I have always had rather a penchant for getting rid of things when done with them, but am not yet done with my Rothena by a good deal, nor disposed to think of anything but using them so long as health & sight are given me. As respects the future I see no competition with you, because I have no thought of letting anybody have them. I fear I should not work so well on them if I predetermined any such arrangement.

Yours sincerely

Edw. Tuckerman

Auckland
11 March 1865

Dear Doctor

I have your kind note of the 8th & 9th inst. & am sorry to give you trouble.

The MS of the Lichens of the Pacif. Exp., is entirely ready — except that I am incorporating portions of it from time to time in my memoir now in hand — which portions will be used to the exclusion of the other, — & secondly, that I shall make a few changes, — the position of Genera, regarding the sections, & not passing any. So I can furnish it certainly at call. (supposing a couple notice at furthest.)

I shall take pleasure in writing at once to Mr Rathbone,

P. S.

In my Herbarium, I exhibited to you in my book the black shale of my geology, without hesitation, as you spoke not for yourself but for Science. I shall not forget your suggestions & have no idea of declining their validity.

C. T.

& giving him all the suggestions needed, & have great hopes that he will accomplish something in Lichens, as this can be got when little else offers; & I need not say that I am glad of the opportunity. There will probably be less of the peculiarities of California — but much else of great interest.

As to any Genera the task is a different one from what I suppose would be a Genera of Mosses. In the latter, divisions are, as I take it, considerably artificial, but not so in Lichens, at least as they looked at them. And trying as I am to set up again these natural arrangements, after the dilacerations of the last 30 years under the microscope, it especially as all the microscopic differences must be embraced & disposed of, no trifle. In fact my thesis covers pretty much the whole ground of Lichenography, above species. And the difficulties of the study are such that scarcely any two writers from an original stand-point agree, as I have. I could not avoid the labour any longer, & must now complete it. The work half will be done, as I suppose, before Summer — & I shall do nothing till this is complete.

Thank you for the prospect that the Academy may be able to bring it out. I need the assistance: but had rather pay the cost, than delay its publication.

Yours Sincerely

Edw. Tuckerman

Amherst
1st April 1865

Dear Doctor

I must beg you to excuse this note, as my eyes are but poorly — after some 3 days of microscopalgia: I shall see Mr. Leighton at once. I was not aware that he had "cut my acquaintance" — & shd probably not have thought of him (the advantage enjoyed when he sent me his Exercitala being quite on his side) had it not been for the letter to you in which he did not appear to know that any body was studying Lichens in this country.

Of course I cannot send him the specimen (in large pack from Leidy's Coll.) nor have I time to recommence

exchanges, except of my Exsiccati, as they are imposed — but he makes no allusion to them.

I shall send him my publications however, & beg for his forthcoming Memoir — & this on specimens from my own field study.

When I go to town in May this is to be sent to the store & my clothing, furniture from the house lately occupied by her — and I fear therefore that I may not be able to stay at Cambridge in accordance with your kind invitation. But come there I shall be sure to.

Yours sincerely

E. Tuckerman

Amherst
6 May 1865.

Dear Doctor

Thank you very heartily for thinking of me in the way of Carex. I have studied those of Dewey with care, & send many to Booth, & my Coll^n is as good as I have been able to make it. And I shall be extremely glad of any duplicates in your hands which may represent species that I have not. Booth had Engl. sp. of C. Torreyi (Carlton House)

I have been delayed by some lectures on History which I have been reading, but shall leave for

Boston on Monday. When there, I have to see about some Income Tax affairs for others, & also about my Mother's House which requires me to go.

But the first moment I get, I will call & thank You for your kind offer, which seems to promise just what I have been wishing.

I could not expect Dr. Boott, to trouble himself about duplicates but I ventured often to allude.

Per contra I have an original oil painting of Mr. Nuttall painted for Mr. Cooper, procured by me at her sale, which I should be happy to contribute, whenever a place shall occur for it.

Yours sincerely

E. Tuckerman

Amherst
18 May 1865

Dear Doctor

I had a hard "spell of work" in Boston, & was pretty well tired on Sunday when I reached your house. So I only looked into the green-house, begged a few cuttings, & took my departure — the hot weather, & my fatigue making it difficult to do otherwise.

So I suppose I must lose the Carices, unless you will give me the opportunity in some other way, for really not

practicable.

I left the Drawings. The MS is quite ready to print, but if the Post leaves it for a year or two yet there may be other alterations to make.

I am now going to take up again my account of the Genera of Lichens, suspended for a fortnight, & hurry it through as fast as I can.

Yours sincerely

E. Tuckerman

Amherst
19 May 1865

Dear Doctor

I wrote you yesterday, & have just received yours of the 17th. I trouble you further only to say that it will be a great object to me to glean the Carices, as my Collection is small — my only opportunities for making it having been my old duplicates, Sartwell's, and collecting.

Would it be possible to let me run over the collection here? If so, & you will tell me under what limitations I am to be,

I shall be grateful for the opportunity.

Your more particular account of what I might have heard, had I been nearer to you, only makes me eager for whatever opportunity may remain; not however to get duplicates, for which I have no use, but adding to my Collection, which is smaller perhaps than you suppose — though finely mounted, & in good order.

I have a number of Reams of excellent paper bought by your advice, & made up to your order, which I shall be happy to transfer to you at the cost-price, when you shall want it. There are eight reams. It is no doubt better than when I had it, & cheaper than it can be bought now.

Yours sincerely

Edw. Tuckerman

P.S.

If you cannot otherwise select Rev, can I do so next 20th of June? I shall then expect to be in Boston. I do not wish to give up the Nuttall picture, till it has an assured place provided. I have more fond one for it, & it is worth too much to remain any longer in a Lumber-room.

Amherst
30 May 1865

Dear Doctor

The Bundle is here, and I have been just back some two years, am in my old Collection again, & getting as interested almost as ever with Cases. For it is as you say, the bulk of the Collection is what I got together, & gave to Boott. And it is these very Specimens that I desire greatly to possess again. But I fear, now that I am half through, that I shall overstep the limit that you meant to set. My Specimens are associated with so many memories, that I wish a great many in some cases. Did you mean that I sh'd resume my Collection, so far as I wished?

I wrote you that my present Coll^n, though mounted much better than the old, is very much poorer for all species collected by myself, is poor as respects others — You will imagine them how gladly I welcome those that once filled places, since unfilled. But I would be sure that I am right, and not overtask your liberality.

There are several sheets containing species nearly akin (as the Scoparia group, &c.) placed together for study — Are you willing that I should retain these?

There is also a considerable Rag of Cyperaceæ exd. Carex, mostly Boott's American collection; but a few European. Those I have just together in a seperate parcel, not seeing sure, as you did not speak of them, that you intended to have them selected for.

If you will tell me that I am, or am not wrong in my eager desire to recover the larger part of my old Collection; or better yet, exactly what of yours neither are in this respect, I shall be grateful.

Yours truly

E. Peckham

P.S. The paper is 11½ × 16 3/8, & cost $3,50 a ream in 1859, being bought of Mess^rs Rice & Keadall.
If the size is right, I will send it, & the Picture with the Bundle of Carex, as soon as I finish the Letter, & hear from you.

Amherst
5 June 1861

Dear Doctor

Thank you heartily for your kind note of the 2d, and for its appreciation of my feelings in view of the collectors' whirl I took so much pains to bring together.

The reason I wrote last was that I cannot feel content with a single specimen — on the contrary — I wish all I can have of some species; a goodly number, of others; in short I am sure I have your entire sympathy & approval (that is of the Botanist.)

let them rest. The whole make 23 species. The rest of the Scirpi &c. were of no especial account, except that I was glad to get, here and there, one of Booth's old Cambridge specimens of our common Scirpi &c — for I study these genera, & collect when I happen to be, though not attempting to make any general collection, as of Carex.

I will put up the Paper & the Picture as soon as I can.

If you decide still to reclaim Booth's sheets of groups — I shall at any time willingly resign them. They contain however a few things (as C. loliacea, C. Magellanica, K.) not now in my coll^n, which I should be glad to have opp^s of studying.

My eyes are weak, & I have gone already into Carex herborizing, and shall try to get C. Backii.

Gratefully & sincerely yours
Ed. Tuckerman

Today I have sent the parcel to Mr. Clinton. It is not so bulky as when it came, but is still a large package — a foot through. For my specimens of European Carices in the Collection made since I gave Booth my first one, I depended entirely on the rejected ones & duplicates of the old Coll^n. When therefore I have taken back what remains of the original coll^n, I have put in their place in the Bundle, the specimens corresp^g in my present coll^n; all well mounted, so that in many species excellent sets of specimens still remain, though I have taken back my original ones.

I shall gladly retain the sheets of species in groups; & expect to study them too. And as you desire I will keep them in fact.

Among the Cyperaceæ Ext. Carex, was a little parcel of very nice mixed specimens from Reichenbach Jun., which I left out of the parcel sent Mr Clinton thinking it possibly of use to you, as you spoke as if you had not examined these species. Among them are Carex brizoides, tenuiflora, Suecica; C. canescens, Suecica; C. Micheliana, Styria; C. dichostoma, Venice; Cyp. juncifolius, Sardinia; Cyp. castaneus, Suecia; C. pannonica, Hungary; Kobresia caricina, Croatia; the rest being, if I mistake not more common.

If you do not want them I will dispose of them as you please — or bring them with me when next I go to town. But if I do not hear further I shall

Amherst
3 July 1861

Dear Doctor

I am sorry to have delayed so long to send you the Paper. But the fact is I had almost given up the attempt: really not knowing how to pack it. I had filled a box with it when the box proved too heavy. At last I called in our Irish gardener, who fortunately knew how to do it. I send 6 Reams, which cost me 21 dolls. The remainder was so-keen up, so that I could not well put it together. The Expressman says it will not cost any more than if you will mention pay for carriage.

The Picture requires cleaning & framing — but as it is after

its further investigation.
That is, I wish to keep it in my own Carico-maniacal custody.

I saw the death of Sir John Richardson. I had not the privilege of seeing him when I saw other men of his days — but I have many times followed his painful narration of the Overland Journey: a journey not a year of the latter he sustained.

Mr. Lippincott is working up the whole coll^n for publication, as he informs me.

Yours Sincerely
L. Pickering

all the portrait which is valuable, & any frame it might have had would be likely to look shabby now. Portraits of such men as Nuttall are likely to be rare. Wahlenberg was another such. Iris told me there was none of him, & he would not have any done.

I am delighted at the prospect of more Carices. They will be most acceptable. And so too will any duplicates which you may be able at any time to spare me!

I enclose three plants, which I wish to place before you. The first is Verbena angustifolia from a new loc[s] in this region, which I found it last month. Please accept this.

The 2[d] is a gigantic Rumex that I cannot satisfactorily, or indeed at all determine. If the specimens are worth anything to you I shall be most happy to leave them in your hands. Otherwise I sh[d] like them back.

The 3[d] is a Carex which I am also quite at a loss about. It is my only specimen, & I must ask you to return it. I made repeated attempts to get more, in vain.

If it is possible you may at once refer it — but otherwise I beg to retain my interest in

Amherst
28 March 1864

Dear Doctor

Your kind letter of the 28th, reached me last night. To refer to its items, all of special interest to me, in the order in which they demand attention, let me say that it will give me pleasure to make my Lichenology of service in a 2d vol. of your Manual, provided you are in no hurry.

I have just to finish my "Genera", and the six years work with microscope, compels me now to intermit labour of that sort occasionally, & to spare my eyes. I suppose the "Genera" will be ready by fall. I am at the end, that is but one tribe, & the Collemaceous Lichens remain to be studied, but both of these will take time. Then I have almost committed myself to the continuation of my Lexicon (two vols of which, & the materials, have long been waiting) as soon as the book is done. So I weigh the fact shall really need rest, & time.

The exposition prevented my replying to the letter — but I suppose he will send his scientists, when they come out. It will however be very interesting some to see the Memoir in the Transactions of the Linnean Society, if you can spare it a while.

Then I in error in supposing that you have somewhere a certain packet of Carex, intended for me? There was a reference to such a packet in one of your letter of last year, but I am not sure that I have not misapprehended, & if you do not remember it, hope you will give no further thought to it.

My pamphlet will give a sketch of my notions of the System; & will itself follow it. It is, I think, very simple, compared to the others; & is a deliberate attempt to shew that "Fries", properly reconciled with more recent Theories, is still adequate to all the phenomena. Whatever I have done therefore, has been on a strong foundation; & I am not afraid of it.

Yours sincerely
E. Tuckerman

W Bolander writes that Prof. Wood, I suppose the author of the Class-Book of Botany, is in California; & proposes to bring out a "Manual of Cryptogamic Plants" — he does not say, but I suppose, of the U.S. I have had no correspondence with the Professor, but am a little at a loss to understand, with my 25 years experience, how he can manage the Lichens; but if he only depends on himself and on compilation, care nothing about it. Bolander declined the application for his "materials"; & family. He and I are fairly afloat on the enterprise of determining the curious rock-lichens of the Pacific coast: and one fifth of the stock required to fill one volume (50 nos) of Exsiccati is here in my possession. It happened that I had some important new forms still unpublished, & so I at once determined to secure these, & at the same time shew what is known of the whole coast & the Mts — & this will be printed here, the nos. to go to press next week. In doing this, I confess I felt a little apprehension that the new cryptogamic author would have to depend on a foreign source for his Lichens; & felt a desire to forestall this as far as I could. It is curious that he should have gone to California. It is the one spot of all others in this Continent, where novelties really seem to be common. But I trust foreign help is not to be brought in!

It will give me much pleasure to examine the whole stock of W Mann's Lichens, if he inclines to let me; and as he has not touched them, it will be a convenience to him, if I throw them into the Genera: and then, if he enriches me with a set, I shall be able to give him what further advantage may be got from my study of the specimens. But I could not do the work anywhere but here. If he letter forward them, I can promise they will be met with open arms — and at once disposed of, so far as the Genera; species ogling microscope in good part. I should like to know his wishes clearly; & to be permitted to pay express charges.

In Dr. Nylander's last letter to me he informed me that I had to expect shortly some rough (âpre) treatment for him, I suppose on acct of my not agreeing with him in a scientific judgment.

Amherst
5 Apr. 1866

Dear Doctor

The memoir on articles on N. Zealand Lichens contains but little of novelty, but enough to make me desirous to possess a copy, if it be possible. If you have an opportunity to procure one I shall gladly pay for it. It shows systematic improvements. The descriptions are fuller — & he follows me in giving up one of his two genera intermediate between foliaceous & crustaceous Parmeliaei. The groups are old ones. I followed the little tract by Stizenberger.

As to the Ranunculus, I have nothing of any white one except what we used to call R. fluviatilis. I got it first at Sanders lake near Schenectady. At Boston my notes say "R. fluviatilis not rare in ditches in meadows & slow streams, Concord!, Newton! &c. Also Charles R., at

It got to be difficult to teach the Grammar as I used to do, in your place & I may say so (as you were the teacher) — but Lectures are really what is wanted: & I mean to try to put together something that shall be popular & pleasing if nothing else.

Sincerely Yours

C. Peckham

the Wheal factory in Natick, &
and very abundant in the brook
crossing the road from West Newton
to Watertown."

I do not think it likely that
I found the plant which Bigelow
reported (in his last edition) to R.
aquatica: & have no evidence that
I ever supposed I had anything
but the common White Crowfoot.

Is it not likely to be the
Doctor's own find? Hale's old map
of 30 miles round Boston would
shew the ponds in Newton. There
is one (formerly called Richards Pond)
near the Worcester Turnpike, and just
within Newton where it joins Brookline,
which I should guess to be Bigelow's
station with little doubt. It used to
be a favourite botanical ground with
Cambridge students.

I can put up a Rean or
perhaps more, if not quite two,
of the paper, & will try to
manage the clumsy tying up.
Some reans got separated, or
rather run together — & so I did
not send them. I will spare
every sheet I can.

I am Professor of Botany,
only wait time to prepare
a course of lectures, to resume
the teaching. Meanwhile Prof.
Clark (Chem[l] Prof.) likes flowers
& takes my place for the
present. You see I wish to
magnify my calling — or rather
to have a definite title. I mention
this because I once gave you
a different impression.

Amherst
16 April 1826

Dear Doctor

I received & thank you for the cheque for 10 Dolls — I am very glad the paper arrived safe, & was satisfactory.

Today I return the n° of the Linn. Society's Journal by Express; not being willing to trust it to the Post. Should you have any opportunity to procure a dupt., it will be most acceptable, as I have to keep up with all the literature of my Study. Mr Watt writes to me, but I rarely to him; he is not a great favourite of mine, though I always do what

I can for answer his enquiries & especially to forward his enclosed letter to others.

I must beg to excuse myself from any teaching outside of Amherst. I have already so many scholars scattered over the country, whose lessons I have to determine, that it decidedly interferes with my other labours. I try to bring them to the point of getting a good microscope — & then (in one case at least) a good deal of mechanical labour is taken off my shoulders.

But above all I have little faith in any other teaching than this; & nothing would induce me to go beyond Amherst. My work is before me — & time is short. I cannot undertake for each except by letter, until I can print. When it comes to the latter, I will try to be of some use, by making matters as simple as I know how.

Yours truly
E. Tuckerman

P.S.

It is easy to collect, but slow work to determine. I find this out continually — & my eyes ergrün can.

E. T.

Amherst
12 July 1866

Dear Doctor

I send the Quercus as you desire.

You have the only specimen of Carex glaucodea; & I have no means of publishing any account of it; even if I wished to, in the circumstances. But, knowing your liberality in placing your collections in the hands of investigators, I desire to ticket & name my specimen, as I shd feel a little discontented if any one else got it out. Until I find more, you will excuse me if consider the specimen as only deposited in your

To
Professor Gray

Cavicetum siccum.

W. Mann has just sent me a specimen, from the Yosemite valley, of a Lichen (Acolium tympanellum) which though common in Northern Europe, and extending in the Mountains, to Northern Africa, & sufficiently conspicuous, has never been before detected in North America. The other three California types are all new, & two of them the most remarkable of the genus. At the East we have (very common) an European species with (Nylander says) exactly the range of A. tympanellum, & only one other, of similar range but a little rarer, both here and in Europe. There is none of the evidence of decadence of conspicuous European species appearing here only on the West Coast — but not yet enough to justify me in attempting to set anything down.

W. Lesquereux has some interesting observations to this point in his California Mosses.

Dr Gravitt's death has much affected me. His great kindness to me, was extended also to my father's family. Sure I am that he was one of the best of men, as of learning.

Friendly yours

E. Tuckerman

Professor Gray.

Having thus stated the case for the respondents (the U.S. Government) I await the further pleasure of the Court.

Sincerely yours

E. Lockerman

P.S.

Nylander has had his hand in so many tropical floras, that they cannot well help going to him for Hong Kong + Loo Choo lichen-information. Japan belongs to the more Northern region + might be illustrated by others. I shall try to do what I can finally think, on the whole, proper; but demur as above to putting myself unnecessarily in barbarous hands E.L.

78

Now I ask you for a word of advice as to Ravenel's letter, + especially if I can name any parties to whom he can apply in Europe or here?

Amherst
23 July 1866

Dear Doctor

This time last the Lichens of the N. Pacific Expl. Expedition are stored in my attic much to my regret, as they ought to be elsewhere, but they are carefully done up for delivery to the U.S. authorities; + I will tell you why I had rather not meddle with them. In the first place there, I know of no one suitable or fit to undertake the determ.n of the collections in Holland as Dr. Nylander — + he possesses already specimens of all the new things.

In the next place there are no duplic-

cates is not a fair case, the specimens having been detached & mixed with other things.

And finally the descriptions are all in print. I append the Graphidaceæ, to Dr. Nylander entire — & he has just his determs in print.

If he has the charge of the work to be done, there is no doubt he would like to see the whole collection — & that his report would differ in a good many points from mine. I should have therefore to leave everything else & make a new study and report, to replace the finished one waiting to be called for. & this I do not desire or intend to do if I can help it.

I have nothing further to publish myself — & it is not my fault if the Report itself is not printed.

Once more — Nylander should have the work — & probably may. If he does, he has everything, new. If not, his collections may better be consulted, especially in the case of new species of which there are no duplicates, than an attempt made to furnish fragments — even where they can be spared.

Supposing everything to be as I have suggested, would it be quite fair to furnish the amiable Fée with a visit of the specimens before my own Report is in print?

I know very well that Montagne & W. J. Hooker expressly forbade this in the case of their Java Lichens, — with good reason.

Amherst
3 Oct. 1866

Dear Doctor

I have delayed to thank you for the copy of your memorial of the Harvey — but wish to assure you that it was not lost to us, though we had not the privilege of knowing the lamented botanist. As he was in this country when I was last in Europe, I never had the opportunity even of seeing him. His works have not

This season was signalized by my first making the acquaintance of Asthenatherum.

I enclose also some notes on a remarkable illustration of the strength of the Elm, which I got a student who brought them to me to put in form, expressly to send to you. We shall be grateful for your remarks upon the case — & Mr. Lyman looks forward with no little interest to what you may say on his discovery.

I have had a pleasant letter (15 Aug.) from Fries, who complains gently of age — which I remember he did nearly 20 years ago.

Sincerely yours

Edw. Tuckerman

been acceptable to me, except the Memoir on over Algæ which I have seen, but the impression I had formed of this wonderful industry & power of accomplishing extensive series of investigations is more than confirmed by what you narrate.

I have always connected with him a Coralline which you gave me roots of; brought, if I mistake not, by Mrs. Gray, from Iceland. This has disappeared from the border where it was placed, but two plants in the grass-bank near (where it was also placed) flowered this season, and might contain ta, in more skilful hands than mine — but have dwindled in size.

My eyes requiring rest, I have been studying Carex for a fortnight — & with greatly to renew my acquaintance with my Mt. Holyoke novelty, now in your hands, & shall beg for its return if any convenient opportunity sh.d offer.

I send herewith some results of desultory studies of our common, introduced Festucæ. I cannot help thinking that I have some things not wholly unworthy of your inspection: not quite "coals for Newcastle". I have numbered the papers & shall hope for your determin.

have spoken of — the various Lichen-work already on hand. Beside the "Genera", unfinished — are 2 large parcels of not yet exam^d Cuba-Lichens — 3 or 4 more not yet determined; — My Exsiccati (two vols of which have been bound & ready, & the specimens collected, these six years); — a catalogue of the species of the Yosemite valley for some publication of Prof Whitney; — and two parcels of N. Amer. Lichens for determination. And there is only one pair of eyes — & for the most part hands, for all this.

But enough. I am eager to get the whole done with; & shall in good time hope you will be pleased with the results.

I am busying myself with whatever the Flora here offers — especially Willows — though I make little progress in adding anything to what I had learned; so far. The Manual is alone, the only help — & a right good one I judge it. — Nor need I add how much I desire to possess the new one. It is a Manual indeed.

Truly yours

E. Tuckerman

Prof. Gray.

Amherst
24 May 1869

Dear Doctor:

I ought at once to have replied to your note of the 20th; but the state of my eyes unsettles my usual regularity. And this is also what makes an insurmountable objection to my undertaking any new work. Eight or nine years of constant labour with the microscope have weakened what was strong sight, and it is now easy for me to overtask it. The preliminary work on what was to have been the last division of the book I have in hand (this work resulted, much to my satisfaction, in enabling me to reduce the said "last division" into a subordinate section of the first) resulted, this spring, in such weakness, that

it is now a month since I have been able to ~~write~~ take my seat at my working table, — that is for work; & even reading has to be limited.. In this state of things I have only to renounce my work — & for the season, leaving it to the late autumn, to finish & revise the manuscript. —

I prepared, in 1848, at your suggestion a synopsis of the northern lichens, the description in which gave the first English presentation of forms. — In a new work the descriptions will have to be my own, & in English, not neglected Latin (so far as possible at least), & the labour, with all the knowledge I have since acquired, will be far greater than before. Then I ought, and it is expected that I shd, make my final report on the Lichens of N. America in my Lichenographia, — and this I must do in my own way & time. It will be of course a different publication from such résumé as I shd contemplate as required to make part of a manual for Students — just as yours over mine differs from your Flora N. Amer.: — but it will be easier, pleasanter, & every way more satisfactory to myself. The study bristles all over with difficulties — each to be considered & disposed of as it comes up. I shall do this (pro mea parte) once for all in the Lichenographia; but I do not desire to supplant such final verdict, by a less considered one now, not, if you will allow me, to supplant my own book. As to Students I doubt if there are many here whom I am not now helping, to the sacrifice of a large part of my time; and I see no reason whatever why they shd not wait for that further help I can give them. If they want difficulties to disentangle, I am sure the Phanerogamia offer enough, let alone the Algæ & Fungi. When these last are worked up, Lichens may all be called for too, as they belong between the first two — but there is surely no near natural relation between Lichens & Musci, nor any significance in publishing them together.

I have said nothing of what I might call

Amherst
20 Feb. 1868

Dear Doctor

I have recd your letter, and Mrs Booth's pleasant gift, which you kindly announced — I shall be most happy to unite with yourself & the other gentlemen in such an acknowledgment as you intention. But as no sum is even hinted at — & I do not know therefore what to say — I must leave it to you to set me down for such an amount as — relatively to the rich men, as Lowell, & Sullivant — is proper for me.

I have acknowledged to Mrs. Booth her gift — & have cursorily examined it — only to find new proofs of the elevated spirit in which Booth studied. It is a comfort to me (as I collected & studied them) that the C. Hornemanniana is finally sunk again in C. fulva — but the merging

of C. rigida in C. vulgaris seems, at sight, more difficult.

It is also an evidence of the latest tendency of the author's mind, that his C. comosa in (in the larger view of the type he was able at last to take) restored to C. pseudocyperus.

As to the Mt Holyoke Carex I am content to leave it for further investigation. But I doubt if it be not more remote from C. grisea than is C. flaccosperma. In both these we have (if I mistake not) oblong fruit — always perhaps pointed, & becoming at length even a little beaked.

In the Mt Holyoke + Pa Carices, (of which last I am just in receipt of another spec" from mem Philad" Mr Smith) we have ovoid, and really pointless fruit.

I will repay you the money paid for Dutch, when I read that for the compliment to Mr. Boott

I wish much for some information on two of Mr Lesquereux's finds in the South which he sent to Europe, & which Bornet described. Of one of them nothing else is known than the very little upon which Bornet found himself compelled to make a new sp. Lately I heard from Mr L. at Columbus, & wrote to him there — but have no reply. Do you know where his address is?

Yours sincerely

L. Pecheux —

"Patriarchal" or not, such matters belong to ladies — & in the present case, Mrs T is entirely responsible for my appearance. Which I think a sufficient replication quoad barbam.

Amherst
8 Mar 1868

Dear Doctor

Thank you much for the speaking likeness of yourself which will be an ornament to my little Gallery. Also for those of Dewey — & De Forest. I am very fond of engraved portraits but perhaps less so of the "cartes", tho', if I went abroad it wd be an object with me to form a collection of the one as well as of the other.

Whatever you do with "Tuckermannia", Nutt., will be an ignoramus but I suspect nobody will think of anything but acquiescence in. The plant has gone out of sight for years — & what you called a Grindelia seem to have usurped its place. I raised the true thing once, but have long lost it.

This matter of genera & species has not so great a charm for me as it once had — nor do I see how any amount of acuteness or knowledge can carry matters into any more definite state than a certain conventional disposition, based indeed on facts + the subjective element or "personal equation", but sustained merely by authority. This is enough for the majority indeed — but hardly satisfies those who think even among them.

That subjective element — mere arbitrariness as it tends always to become — how impossible to eliminate it — & yet how difficult to be content with it!

But the bane of the order I am now studying. The history of Lichenography tells how (& considers it a mean reckoning) over four thousand species or sixteen known. But the figures have in fact no shadow of value — representing as they do all sorts of judgments & all kinds of facts. It is strange that this does not occur to him, but he really expresses only satisfaction with his results. I have often been asked for similar estimates, — but never even tried to make them. This Order is perhaps an exceptional one, which writers will always differ about, except as now & then men like Tuckerman step it up and give it shape for a generation, but my thought has, if I mistake not, value in itself.

Sincerely yours

E. Tuckerman

Amherst
4 May 1866

Dear Doctor

I have never attempted to collect "Carex". My collection embraces beside Prof. Lincoln & Drs. Sartwell & Robbins, & Oliver & Wright — only eleven, all Lichenist. Receiving several, it occurred to me that I should like all the lichenologists — but Mr Lander preferred to give me an exceedingly decrepit head of Montagne — and two others, to whom I applied, gave me nothing — so I tried no more.

This accounts for my never having asked a copy of one of your portraits. If you will let it share a place in my very humble gallery — I shall be much pleased to have it, and, as you suggest, in more than one form. And I am glad to find you what is considered a good one of myself.

I heard from Fries lately (4 Nov. 1865) when he appeared to be well though calling himself old & only able to enjoy botany in print.

will I hope go far to meet your views in reference to a Lichen-part — of the Cryptog. volume which you design to add to the Manual.

With time I will certainly do all that I can — & whatever I do, if not itself available for your purpose, can doubtless be made so.

Yours sincerely

E. Tuckerman

Professor Gray.

I shall be happy to get Engelmann's Juncí by Express: but feel that I have done very little to deserve a copy: His labours was great — in settling some of his conclusions; & I shall try to settle my Amherst species, when I get the normal collection.

My Genera of Lichens is in it's last chapter — but the revision may be in itself a considerable work — as I have included a good deal of matter which belongs rather to a "Regime of your species" — this I may exclude: suppose I ought to. I wished to make at the same time with my Expose of systematic views, a Report on what is known in all the more difficult — or nearly the larger part of our Lichens Flora, & this has cost time & space — though the long talks on the changes I have proposed in the Arrangement — took up enough.

As soon as this first book, is done I will at once go at a book of Species in some form — & should like also to give a series of popular articles, with plates, — application for which has been made by the Editor of the Naturalist. If I can get the drawings made & if, this may be a useful work, & it will give me a chance to support, in figures, my views of the System.

These proposed undertakings

Amherst
3 June 1868

Doctor

Many thanks for your note just received, & for your justification of my crudely expressed notions on a topic, which I look at mostly from a very limited field. As to the present to Mr Root it will be most grateful to me to bear a part — and am content to leave it to you to ajust what you think will be proper.

I wish much to say a few words on a topic which now & then makes me a little uneasy. You wrote to me last year in regard to my preparing a part (Lichen) in a cryptogamic volume, as I understand, to make Vol. II of the Manual. And I replied, setting forth

of course I do not imagine that you would consent to anything of the sort — & yet it is the only alteration I see, unless I shall be the man.

Now Mosses are so utterly diverse from Lichens — that I feel as if I wished to come out first in propria persona, & show that I can walk alone, when I shall be proud & happy to join hands with such excellent cryptogamists as Sullivant & Lesquereux.

At the same time I do not wish to give up the field to any foreign savan — or to his system.

Will you not see if you can make the affair so that I can have my chance. You say that Sullivant & Lesquereux are soon busy at their part. Is the publication then imminent? I shall at once desire to prepare some sort of handbook, the moment, the present work is off my hands. And I wish to satisfy you in all respects. Sincerely yours,

E Tuckerman

that, under the circumstances, the proposition was not so pleasant, as no doubt it ought to be.

You replied that then I & No say who could do it — thus implying that you were decided on having such part elaboration by somebody.

Now in regard to the fact, please to look at my position. After years & years of study, the last nine almost wholly in the tropics — I have got so far as to undertake and succeed in reconstruction of the system for myself. — This is done, except the elaboration of 3 genera only, & the necessary revision of the whole; & I repeat that the thing is a successful development from any point of view — such is Bth's & the last 30 years' results) & will therefore take its place among the other draughts of the System.

Now as at once simpler & I am persuaded correcter, than Nylander's, I am naturally desirous to have the field here to myself to bring out a hand-book, which shall display the System, & give it a chance; as against the genera- & species-mad European authors. And this brings me to your 2nd enquiry, who will do it if not I.

I know no one here who can, unless he works in connexion with Nylander. But what an injury would it be to me, & in any apprehension to Science, if I were thus supplanted, after so many years of hard work!

Vauthoria above-cited; claiming priority on the score of the name alone, though never uttered in any other than a wholly subordinate sense.

Now I have no hesitation in regarding Norman as the author of the Genus, and he assuredly is, his pamphlet now at my side having 9 years priority of Th. Fries's — & if the author, his name must be accepted. Am I not right?

Then as to varieties of species — Nylander found a lichen near Paris, which he took for the Norwegian Biatora fuscescens, Sommerf. This it was better seen not to be by Anzi of Como, who called the Paris lichen (I find it also in Cambridge) B. Nylanderi. But Nylander meanwhile, or perhaps at about the same time (it is unimportant) suspected the full correctness of his reference, & accordingly distinguished the Paris lichen as B. fuscescens v. leprodea, — & supposing this to have appeared in print before the B. Nylanderi, Anz., there are those who pretend, that if the species be taken up, it must be "B. leprodea, Nyl." This amounts to saying, & so in every other case of the sort, that if an author distinctly publishes a thing as belonging to a certain old species, from other states of which he happens to fancy to slightly distinguish it by a variety-name, just as he might distinguish other states, with no trouble at all — & this form of an old species is then taken in hand, newly analysed & set forth, with a good deal of trouble perhaps, as not the old, but in fact a new species, we are yet to be bound to use the variety-name for specific, & put the author of it for author of the new species.

I cannot assent to it, whatever Congresses may decree. The importance, practically, of the distinction of Section & Genus, & Variety & Species, and the subordinate, & unessential character (comparatively) of the former of each of these classes it to me clear, & of clear importance in Systematick, as the Germans call it. And I hope I have your favour.

Sincerely yours
E. Tuckerman

If Dr Chas. C. Abbott [illegible] Dr. Torrey has seen [illegible] noticed the reception of specimens sent to him, especially [illegible] I wish I knew where [illegible] to send [illegible]

Amherst
7 Aug. 1868

Dear Doctor

I enclose a cheque for the amount of my proportion of the cost of the Longfellow, & doubt not that Mr. Booth will get a nice copy, while wishing heartily that it will give her the pleasure we intend it should.

And I am most happy & proud to contribute my photograph to Mrs Gray's American collection for the benefit of de Candolle, & would offer it to her with my best regards & many wishes for an agreeable voyage & tour.

It is very satisfactory to me & need not also say, that my Carex has at length passed its examination & been admitted. Mr. Olney's last suspicion (founded on one of Prof. Porter's) that the plant is after all nearly associable with C. granularis, is in my view an important point in its favour.

4

And now, if I can only prove the lucky woodsman to recover C. Torreyi I shall be quite content. For it may as well be common to East Pa., & this peculiarly situated region of Mass^{tts} as C. glaucodea.

Thank you also for the rest of your latest reviews of our Botany, & for the "Laws of Nomenclature" adopted by the Congress at Paris. I have never supposed that the character of this Assembly was one which gave more than, at the most, an advisory authority to its conclusions, & was not surprised that this was, as I think it has always been, your view. I read all the proceedings at the time, but without feeling impressed that I was reading more than the carefully expressed opinions of a few botanists not many of whom were widely known. Some of their opinions were I confess not quite clear, but the translation, & your commentary has thrown light. But I do not now know whether or no, one case is provided for, & this I wish to submit to you, having already expressed myself, an opinion on it, in print, which I am not at all inclined to recede from, though it be in opposition to a common practice.

To give all the facts then, what I expressed in print, was this:

"The last-mentioned lichen appears (L. spitzbergii) to have been first recognized as a species by Wallroth., ... and this being so, should bear the name which he gave it. The fact that Acharius called the same plant L. Wahlenbergiana var. truncigena can give no precedence to the later specific name, L. truncigena, because the Acharian designation expressly asserts the exact opposite to the latter, namely that the plant, instead of being a new, is an old species. And, if this opinion is not mistaken, we may venture to say, generally, that the name which may happen to be given to a variety has no precedence, but may be adopted, or not, if the plant is taken up as a species." (Obs. Lich. in Proceed. Amer. Acad. 5, 415.)

Just so with sections or Genera, &c., of which the following is an example. Under the name of "Xanthoria" the yellow Parmeliaceous Lichens were made a section of Parmelia by Fries in 1825. But he dropped it in his great work of 1831, & has never since alluded to it in print. It proved however, when the microscope "found-character" were taken into account, that these yellow Parmeliæ were very distinctly limited in the latter respect also, & so in 1852, J. M. Norman of Christiania, set up the genus Teloschistes, & accurately defined it as generically diverse from Parmelia. Next, in 1861, Th. Fries did the same thing, but adopted for his new genus, the sectional designation

Amherst
18 Jan. 1870

Dear Doctor

I have heard today from a gentleman (Rev. Mr. Pierson), whom you directed to me, & was sorry that I could not send him a book at once; or tell him something better than that the last general work on the Lichens dates from 1814! The day I get the Genera off my hands, I hope to set about a hand-book to be put together at once; despite all theories & doubts.

There are ideas, genera that need more monographical study

exactly what Schweinitz arrived at & expressed. A Lichen indeed; in which however an Alga officiates regularly & always in the place of the proper gonidia or assimilative system of the Lichen!

Mr. Ravenel has found Juniperus communis near Aiken; & has been greatly interested by the discovery. If he desires specimens, I got Prof. Goodale to supply him.

Sincerely yours
E. Tuckerman

Prof. Gray.

P.S. From what Mr. Mann told me (by letter) I don't if all his Lois coll.n of Lichens is in quite satisfactory order. That, in part at least, the determinations are a little confused. I hope therefore that no specimens may be sent out without the collection having been carefully gone through once more. Otherwise errors can scarcely be avoided, as Mr Mann seems not to have quite finished his work on the plants. If you have opportunity to suggest this, I shall be grateful.

before they can receive justice, & there will not be time for this before I get together my hand-book — but they will be less important in a book for students. And meanwhile I do my best for all enquiries, by naming their sets, & thus introducing them to the system. This work takes up not a little of my time, & the number of those who avail themselves of our help is a comparatively large & increasing one.

Prof. Schwendener of Basle, with whom Prof. De Bary also goes, has started lately a theory that the green cells (gonidia) of Lichens are (not merely like, but really) Algæ, & that Lichens generally are only a division of Fungi for which certain

Algæ officiate as, so to say, Nurses. A very curious plant found by me at Cape Ann bore so manifestly on this theory — this plant offering the whole habit & indeed structure of Lichina, except that the place of the gonidia is taken by an evident Alga allied to Mastigothrix — & never occurring otherwise either at Cape Ann, or at New Bedford (W. Valley) than in this relation to the Alga — that I sent it to the Professor at Basle, & he has fully considered it, with whatever of the facts look the same way, in a recent memoir, not, I suspect, generally known: but — to which I cannot but invite your attention. The opinion reached by me, that 1, this plant was a Lichina (however an a genus, unknown to N. Amer.) but 2, so differenced as to require the at least provisional denial of the first opinion, & the placing of the thing quite separate as a new, compound organism, an-

Amherst
7 Feb. 1870

Dear Doctor

I am in receipt of your letter written conjointly with Mr. Wright, & coming as this did just after my (still unacknowledged) communication to you, detailing somewhat the trying embarrassment (+), which the claims of correspondents, & of my own work, have brought me under, I can say no less than that it was for bounds breaking me down, and inducing me to think seriously of abandoning my studies.

The Cuba work was offered to me, & accepted in good faith, but it proved very difficult; & I found at last that the

(+) This account of the letter is coloured by my present anxiety — but I mean nevertheless to make it clear before me all I could do.

But I do not mean that Mr Wright shall suffer, & am willing to take his collection at their value, at any rate.

As to the N. Pacific affairs, so-ever was said to me & nothing was done, & the whole collection was ordered for transmission to Washington. And I am sorry that you deem an imperfect set. Many of the most interesting & new things hardly afforded specimens to me, — & though I am still averse to giving away what I have, I know of no place to be thought of for the final home of my collection, so proper & acceptable to myself as the Harvard Herbarium.

Now I wish to call & talk about these Cuban matters, & my own route, first with you, & then with my excellent & most esteemed friend the Cuban botanist — & hope to feel a little brighter after it; for I need to rest. I expect soon to be in Boston, & suppose I can find you in the mornings. Sincerely yours

Asa Gray

latter portion could not be done here — & could not be well done anywhere unless by Dr Hyde Baker — of Paris who has made so many species, & settled to many old ones, in these two great tribes, that one cannot touch the tropical representatives of them without having his types at the finger-ends. Accordingly I proposed to him, soon after, or at about the time of the publication of the first half of the Flora Nicaur, elaborated by myself, — that he shd elaborate the two remaining groups, & he declined so positively (I fancied & felt so, on account of his being unable to afford the time required, gratis) that the matter came to an end. Since, though feeling unsatisfied with the condition of

(*) The first part of Mr Seemann's new flora ready for transmission these five years &

things, I have consoled myself with the thought, expressed, if I do not greatly mistake, to Mr Wright, that when it could be permitted to go on no longer, I would merely pay him the value of his collections, & thus obtain them whole stays.

It occurred to me however, some weeks before your letter came, while I was working through the last of parcels accumulated during the whole of the past year (from American correspondents) that I would renew my proposal to Dr Hyde(*), & I have done so — though the letter was not written till after yours was received. If I can prevail upon him, I shall be delighted — but know not what my reception may be, as I have not corresponded with him for some years. He would enjoy the work — but if his means of living are no better than six years ago, perhaps he will not think he can afford it.

Amherst
18 Feb. 1870

Dear Doctor

I have just been in town, but in circumstances which quite indisposed me for business.

I send you, by Express, a proof-sheet of the Letters of the Japan Expn, I think they will reach you without injury.

All right, in a letter accompanying yours, I accepted of his offer to take the Cuban Letters still in my hands, at their value, provided I cannot get them determined within a reasonable time.(x) I supposed him to be willing to do this, & to rely on it in fact — and should be much happy if the business were once well over, either way.

(x) Mr Peabody of Salem

But I am sure it is very desirable to have the tribes remaining determined by the one thoroughly competent head — the monographer of one of these tribes — is as good as that quite, of the other —. I shall take no further steps till you allow to him has had full time to reach & be considered by Dr. Nylander.

Please to kindly inform Mr Wright for me, & believe me

Yours sincerely

C. Tuckerman

Professor Gray.

P.S. Hepp is the only important collection, not now incorporated in mine, but I have the very useful "Abbildungen der Sporen" which accompany the work, & am not now easily to be got separate. I sent for the work in fact, but Simplin mistook & sent me only the Abbildungen — & I never sent again, knowing, I suppose, almost everything belongs to it.

C T

Amherst
16 Dec. 1873

Dear Doctor

It was with great pleasure that I received your Botanical Contributions, especially on account of the superscriptusa; and the kind note which followed was most acceptable. When I went away I could not read, and could write only with difficulty — and that must account for any lapses in the note that I sent to you. When I say I could not read, I mean that I could not read at all any contemporary passage that I had not myself composed, for more than a minute and a half, but could read what I had myself written a little better. The brain will not work, in that way. Now I had been a year in this state when I went to Europe. There I gradually — how gradually! got better of this, and the walking-power was also in the same gradual way restored.

I met an old army-Surgeon who had seen many Sun-strokes in India — at Lugano; and he advised the strongest attention to exercise — despite the feeling that I could not

go about without somebody on my arm — and accordingly I undertook the ascent of a very steep & naked mountain (Sta Salvador) of 3500 ft — the difficulties of which were that the bareness of the sides exposed the whole country around — & gave every chance to dizziness. I got up however by avoiding all looking about; & when I came down could do it & look up the easier too. It seemed a turning point (Good Friday 1873) & I have gone on ever since in the same good direction.

General health had been improved the year before I went out, & is now excellent; but I cannot undertake any close work without painful accession of blood to the head.

The "Synopsis" is therefore still where it was, but I am trying to get a hand-book put together by a friend (name not to be told till he is sure he can do it) with the help of my herbarium &c. Such a book is needed, but the difficulties are vastly increased by Dr Nylander's constant construction of new species on (to my view) wholly insufficient grounds. It looks as if the Arrangement of the Lichens to become impossible to anybody but himself — and in fact that the study of Lichens would end in collecting and sending the Specimens to Him. This "species-krämerei", as the Germans call it will end ill enough I fear, for our study — unless some one shall follow Nylander, and turn his immense accumulations to the service of a reformed Lichenography — a reproduction of Fries's work of 1831.

How great a work you must now have before you! I hope you will often seek rest by voyages abroad. I am strongly of opinion that the voyage is itself an important aid to health. The Chief Justice of England has had the worst case that I suppose ever English judge had before him, & he has kept himself up, by getting into his yacht every Saturday & spending the whole time till Monday on the sea. The Cunards promise daily steamers the coming season. But I am averse myself to Newcastle — it is easier however for an invalid to talk about matters of this sort than anything else. Grey excepted.

It must have been a great blow to you to lose Doctor Torrey. And M Sullivant is also gone, but much more advanced than I supposed.

Very glad you like the looks of my book; I wished it to look neat.

Sincerely yours

Edw. Tuckerman

Amherst
10 April 1874

Dear Doctor

Having been away a few days, I only got your kind letter day before yesterday. I have been occupied all winter with the work interrupted by my illness in 1871. Not a few collections had to be studied, and among them a parcel from the herb. of Dr. Torrey which he had directed the curator to send to me; I hope he did this after the receipt of my unfortunate note and picture, in which case I will take it for evidence of his confidence — as I have no other. He did not reply to my note or acknowledge what (at your suggestion only) I sent.

The little aquatic plant is out of my range of study — but I may just throw in other words, and say it is no lichen, nor of near affinity to any known genus of lichens. The Conferveae Coenogonia are not aquatic. I find the filaments jointed, which must limit the enquiry as respects Alga, but can go no further.

It appears to me that Nylander has taken too slight notice of the real theory of the compound nature of lichens — considering the place he assumes to fill, & measurably does fill. His

is done as well as I can do it. Every thing is studied anew, and I find no little reduction necessary, not so much in what I have ventured myself, which is comparatively little, but in what is now generally received. — Nylander is a great embarrassment. His time is given, not to the bringing together — the formation of comprehensive natural groups, which was Fries' method — but to the sundering of those we have, & the publication in our & the way serials of lots of new things, not seldom imperfectly characterized, often only chemically, & specimens of which are scarcely if at all to be got. A follower of his says to me in a letter (the writer) still unused that he is disinclined now to study any longer his own European Lecanoras, Lecideas, & Verrucaria — on account of this "multitude of new species not seldom built up on anything but essential characters." So that already the determination of the Lecideae of Europe has become nearly impossible for any one but Nylander himself — his descriptions being too often insufficient (from the German) This will make my work on the crustaceous groups hard indeed, and in part a running fight with the man who cannot bear opposition. When I get fairly over the difficulty I have mentioned, I shall do all I can at it.

I have rec'd the learned Dr's determ'n of the Graphidaceae of Cuba at length, and await now only his elaboration of the far easier part that remains, to finish up the Sets of the second fascicle of Lich. Cub. And I am hurrying him as constantly as I can. The sets will meet with a ready sale.

Sincerely Your
Edw. Tuckerman

attitude scarcely will correspond with the purely objective one of the German enquirers, and such language as "ne sont que de jeux enfantillages" is too much like scolding (it is from a letter however, though the little he has printed has a similar tone.) I took with me abroad everything that had then been printed, expecting to study it out in the near neighbourhood of my correspondent, Prof. Schwendener, but it was not to be, as I could do nothing of the kind. I have read nearly everything however, except some papers in Bot. Zeitung, & Bornet, in the Annales. I am far, very far from considering that much has been determined. We cannot depend on M. Bornet's figures, or any others, however inclined to. There is too much room for error. Nothing will serve but the accumulation of evidence — that there is really no genetic connexion between the bulk of a lichen (the "medullary" tissue) and the gonidia of the same. All we know so far is that two or three observers declare they can find no such connexion; — and the curious, medial nature of these plants, ("Lichenes, quoad vegetationem ad Algas relatos, quoad fructum Fungos esse," as Fries declared in 1825.) It looks now as if Lichens would, systematically, hold their own in any event, their relegation either to a class of Algæ (Fries) or of Fungi (others) being improbable. Schwendener put first in his principal memoir a little maritime lichen which I discovered at Cape Ann, and which might at once be referred, by all other characters, to Lichina, were it not that the place of the gonidia is taken by what is clearly a microscopical alga. I found it impossible therefore, as this noted fact is characteristical of the plant, and equally so of the N. Bedford plant afterwards found by Mr. Willey, — to give the thing any name or place whatever; and referred it to the well-directed microscope of Schwendener. He was only too happy to get it, and made good use of it for his theory. Now curiously it is true that the gonidial chains of Lichina do here & there make their appearance in our plant, and had been observed by me; but do not characterize it, as does the locum tenens. Schwendener has no difficulty in calling the whole plant a Lichina, the gonidial system of which was constituted by a Rivularia, of which latter-alga the gonidial chains were simply a modification. This then, and all the Lichens were compound plants: and we must make the best of the startling anomaly that one plant could act as vegetative or nutritive function for another — one organism serve as liver &c. for another. For myself I await further evidence generally, and, especially as regards our own curious thing, which, I prefer to suppose will ultimately turn up without the Rivularia. (Should you desire I will send you drawings, of Schwendeners memoir at any time, & I need not say the plant itself.)

A tendency to congestion, rendering stooping the head injurious interferes still with my work, but the general health has been remarkably good. Could I get over the named difficulty I should go on rapidly with my Synopsis (or however else it be called) of which the first Family & the larger part of the second

Amherst
7 May 1874

Dear Doctor

Accept my thanks for the excellent picture of Dr. Torrey which I shall preserve with care. It exhibits him in his best days. It is a fine head. Thanks you also for your kind reply to what I said — & I read it both prepared to assent to the whole. Not a word more can I say. You did send some years notice of him to the Smithsonl. I need not tell you the interest with which I read both.

I fear I can give you nothing about Mr. Russell. The dates I do not know, and I have no gift for such notices.

the first book of the sort supplied to students by me — the earlier Synopsis (1848) having proved of no little use to our few lichenists, and as offering the first English version of Fries's admirable diagnoses, deserving of remembrance. Mistakes comparatively very few!

I am thankful to see that Dr. Farlow is also lichenizing — but hope fervently he will preserve a sober judgment — as to species-limits; and this is now perhaps difficult to do, in Europe. But Dr. Müller seems to be one of the sober sort.

Truly yours

Edw. Tuckerman

P.S. The remarks on my earlier books were inspired by something you said in your notice of the "Genera" in Amer. Journ. Sci. shewing that (and no wonder) you had forgotten the first one.

When a list of Dr. Harris was wanted I had to decline, and blundered, I fear, even in the few words that I sent to Mr. Higginson. I spent weeks, thinking over & trying to put together something at Mr. Russell's request — but doubt the satisfactoriness of what I did make out to do, as it was never acknowledged. I cannot write such articles. Mr. R. was a real lover of nature in its stalk forms, and a pleasant companion in the field. He was also the first I suppose in this country to use the microscope in systematic study in lichens at least. But his means, his correspondence and his range were much limited, so that he could not then venture to consider his species ultimate, so to say. He was always a very kind friend to me, and I remember him with affection & respect; but do not know how to say more. Indeed, I knew him mostly as a correspondent. Mr. E. B. Wilson of Salem, his minister, was to use my few notes in preparing a discourse — but I do not know whether it has been pronounced. But the notes were hardly used as would be of any use to you, if I had them.

I shall do my best this summer to settle my health by what is best & all for it — out door work on my place. One week or so has had already the best effect. And I assure you I shall try hard to carry on my work on the Synopsis as rapidly as possible. That would suit me that it will not be

Amherst
3 Nov. 1874.

Dear Doctor

When appointed to take charge of Botany here, I immediately undertook a survey of the region, with a view to a new Enumeration of its Plants. Dr. Hitchcock's Catalogue of 1829 was the basis, & I undertook a new study of all the more difficult groups of Phanerogamous vegetation — Salix, Solidago, Salix, Quercus, Juncus, Cyperaceæ, Gramineæ, &c. to the end named. All this had in fact been done for years, — Lichens this spring. I was informed of a strong desire, both in this & the other College here, that my Catalogue should be published, as soon as it might be, for students' use. I at once took it up and in about a fortnight had the principal part ready. Dr. Hitchcock had included however the lower Cryptogams in his list, and I felt it desirable that new Enumeration should be made how to accommodate (for our Catalogue) the terminology to that of the other parts of the Manual. For myself I have Endlicher & Sachs, before me, & shd be happy to follow the latter, whose arrangement inverted, to save you the trouble of any reference, is

Class 1, Dicotyledons
" 2, Monocotyledons
" 3, Gymnosperms,
" 4, Lycopodiaceæ
" 5, Rhizocarpeæ
" 6, Ophioglossæ
" 7, Equisetaceæ
" 8, Filices
" 9, Musci
" 10, Hepaticæ
" 11, characeæ
" 12, Fungi (including Lichens!)
" 13, Algæ.

Now, as all this may suggest many difficulties to you of which I know nothing — & to discuss which you cannot give time — will you kindly tell me how far, and, with what change of form I may follow it, — or give me another solution of my difficulty? I have quoted Sachs's method merely as the newest — & do not at all agree to his relegating the Lichens to the Fungi, as you may not to other parts.

But the Catalogue is required, and all but ready — the copying in of Lichens & Fungi being alone left to do — and I am stopped by this difficulty of arrangement!

My health has gained, and I have been on the Lichens for some six weeks; hoping to accomplish a good deal this winter. I hope for a word from the midst of your labours. Sincerely yours
Edw. Tuckerman

of these. Happily this proved practicable. My old friend Chr. Frost of Bratteboro' is the well-known, careful cryptogamic botanist of our valley, & at once consented to furnish the Characeæ, Musci, Hepaticæ, & Fungi, which, with the Lichens, & myself, enable me to furnish a very complete view of the Botany of our region, Algæ only remaining unstudied here, an almost evergreen. The List is thus all but ready — and yet a difficulty occurs, which ~~me~~ I cannot undertake to settle of myself.

The Manual is, I need not say, our Manual; & ever has been since I received my appointment. But it does not include the Lower Cryptogams — and the difficulty of arranging these conformably with the rest of the work, is where I entreat your help.

Now Cryptogamists, so far as I know, regard Musci, Hepaticæ & Fungi & Algæ as constituting Classes, of which, orders, so far as used, are subordinate divisions. And these Classes are to be understood, I take it, as equivalent in value to Characeæ, Lycopodiaceæ, Filices, and Equisetaceæ. If you accept this, and the arrangement of the Text-book is not perhaps wholly unfavourable to it, is it possible to look at these Classes as equivalent in value to the respective Orders in the phanerogamous plants? Can Fungi & Lichens relate, in that way, to Juncaceæ, or Rosaceæ, &c? It is to my mind impossible; and so I do not see how I can call Lichens an Order; and, in that case, I am equally unable to see how I can call Equisetaceæ one.

And, if you favour me so far, and will not object to cryptogamists settling the names & their divisions for themselves (of which I am sure) will you shew me—

Amherst
9 Nov. 1874

Dear Doctor

I thank you for your kind letter. I fancied that you might be prepared now to make some changes in the scaffolding of the System, but find you are not — and have nothing left me but to conform. Indeed I find no difficulty (as respects my branch) except in the continuous numbering of the orders all through, as if Lichenes, Algæ & Fungi were of the same rank as Ranunculaceæ & Rosaceæ. But it occurs to me now that, for my list at least, this continuous numbering may be disregarded, and the question left open. It might be added that only Lichens & Algæ together should be equivalent to Fungi, or to higher division referred to. The differences of Fungi are so prodigious, and they stand so alone in vegetable nature (however analogies be shewn to exist in the fungous Phanerogams, as by Grisebach) the whole vegetation by being too difficult to permit of their being put with the Mosses & Hepat.; shall I make of them an intermediate class? Or will you consent to place them under Anophytes? It seems to me, considering as I do that the beginning of vegetation (diatoms) may be said to rise into higher Algæ — & to relate to a yet higher & central group (Lichens) which — manifestly descends till it disappears in a third division (Fungi) that there is no place for Characeæ in Thallophytes, unless they find their home definitely (which modern specialists do not allow at present) among the Algæ. And so, I shd like, if I may, to make Characeæ the Third member of Anophytes, — their name not involving the difficulties of Muscineæ, Sachs. But I know not whether it will do.

Nothing but my extreme deafness has kept me from renewing our old invitation to make us happy by coming to Amherst and to us. It would indeed be pleasant — and I shall hope for it whenever you can get away for rest & recreation. But I wish I could hear better!

Before my last voyage home, I did remember your wishes as to my Lichens, and think there will be no trouble in regard to the matter, when I resign the collection.

Prof. Gray.

Sincerely yours
Edw. Tuckerman

that it is not easy to connect them with any of the groups — and, on the other hand, Lichens & Algæ are so intimately related to each other that it is impossible to view them as other than in closest connection. This indicates my notion that if Orders 1, 2, 3 is prefixed to the division of Thallophytes, it will not express the same values as in Phanog. Plants. We cannot now follow Fries in referring, positively, Lichens to Algæ — any more than any late author, in placing the first-named under Fungi. But, for me at least, the fact that, as regards fructification, the Fungi not only repeat but surpass lichens, weighs less than the admitted close relation of the latter, in the vegetative system, to Algæ. I place them the last Class, in your arrange[t], as follows.

Class V. Thallophytes.

1. Algæ
2. Lichenes.
3. Fungi

The n[o] not meant to assert difference in rank — the lowest vegetables ascending finally in Algæ (protophytes) to Lichens — which descend, on the other hand, as the vegetative system disappears, to Fungi (hysterophytes.)

But I beg once more for your help. Endlicher did not hesitate, apparently, to give Chara & Nitella a place in Algæ; & you, & now Sachs, see a good deal looking in favour of the view. But the latter points out that the spermatozoids of Chara &c are not like those of the Thallophytes, but those of the Anophytes. Now I do not know the value of this note, from the spermatozoids, upon which Sachs makes his Characeæ a division equivalent to Musci, Hepaticæ, Algæ & Fungi; but the figures certainly exhibit the resemblance he states. Shall I then place Characeæ in the Thallophytes, as a sort of precursor of Algæ, perhaps one day to be put back into Algæ, or,

Amherst
22 June 1875

Dear Doctor

I am very sorry to hear that you had been less well than usual during the past fearful spring — & had gone south. I trust you gained the needed restoration, & infer as much from what I have heard about your having at least thought of coming to Amherst. Let me renew the request so often made that you, and Mrs Gray will make our house your home when you do come, despite any troublesome incapacity for hearing — and especially that you will give us notice, that we may not be absent, or ignorant of your intended visit.

I send with this a little way-side plant for which I seek (for my Catalogue of the Amherst Flora, now nearly through the press) the name you shall give.

I had taken it for the Red Campion of England, & following Hooker, my old authority, had referred it to Lychnis dioica, L., without reflecting that this seemed to imply a setting up of an opinion of my own, which was not intended at all.

The plant blossoms in the day time; has bright pink flowers (never otherwise) & these flowers are (testibus Mrs Tuckerman, and other ladies, fully competent) quite inodorous. I have observed it, for years, about our roadsides. Will you kindly send me the nearest line of direction in the matter; & oblige

Yours sincerely

Edw. Tuckerman

Amherst
28 June 1875

Dear Doctor

I thank you for your settlement of the Lychens, & for saying you will come & see us.

I should very gladly accept of the Lichens (& specimens of them) provided you will not expect too much in the way of determination; the region being almost terra incognita with Lichenists — only what we got by Hooker having (so far as I know) been studied by any one, & that only by Dr. Taylor, & therefore probably needing to be looked at from a more modern point of view. But I will do what I can.

Truly yours
Edw. Tuckerman

Prof. Gray.

Amherst
31 July 1875

Dear Doctor

Many thanks for the vol. of Flora. My visit to Boston was not so satisfactory as regards the Lichens, as I hoped it would be. I had first to find the Kerguelen Lichens which Taylor described, in his herbarium. But out of the medley of unarranged specimens in the working room of the Nat Hist Society's herbarium — I could only discover one little paper of these specimens, & but one of these of account. It is a wonder to me where Taylor's Lichen collection is — for it could not possibly be that the few plants found by me were all that he possessed. The young man in charge who went through the Cupboards tried to find everything, but with only the result

The specimen most of all wanted did not turn up.

I went therefore with great interest to the Acadey, & found the volume kindly sent by you. Here I was to find Nylander's note on this missing specimen. But, alas! the very ms which contained the article needed proved to be absent from the Cambridge copy. I looked them, as a last resort, in the Acadey copy, & found that the whole year was missing (1858) though 1859 was there.

My copy began as long ago as 1842, but I discontinued it, & did not resume till 1859.

Now I cannot do without this paper of Nylander — that is to say his note on Taylor's new species — and hope you may be able still to direct me to some other copy that the necessary information may be gained.

I sent back the volume of Flora at once — & perhaps surprised you with its speedy return. I regret that you had the trouble & processing it, in vain.

I hope that you will not let me encroach on your valuable time. A word of direction will be enough for

Yours sincerely

Edw. Tuckerman

P.S. I could not go to Cambridge while in town, but some the less enjoyed your kind invitation. But you do not know how hard it is to talk with me. E.T.

Amherst
27 Sept. 1875

Dear Doctor

I found your kind
note on my return from a
fortnight's absence. I had meant
to go to Mt Desert, but
went instead to the Gen.l
Hospital where I was treated
by Dr. Hodges for a painful
disorder — The voyage, successfully,
though it will take a month
or two to be quite well again.

I am quite happy in your
appointment of the Catalogue

& that you too think that I have good specimens.

I may assume to be the author of Nuphar Advena, but Calystegia Sepium was given upon me by another teacher here, on the ground that it violated some rule, to which it will small S. Louisiana was perhaps indifferent or had a different rule, & seems to be generally followed (as far as my [illegible] goes) in writing sepium — but Sir W. Hooker gives Sepium (Brit. flora) & so I followed him.

The "Connecticut Valley Botanical Society" meets at the Mt Holyoke Seminary, So. Hadley (about twelve miles South of us) on the 6th October, 11 A.M. I cannot of course be at the meeting, but we should be very happy if you will come & see us here: & if Mrs Gray will also come.

The Kerguelen, Lord Lichen, have finished last August, and only required the copying of the MS. which I will soon attend to, & then return the specimen.

Sincerely yours

Jos. Dalton Hooker

Professor Gray.

Amherst
6 Oct. 1875

Dear Doctor

You have actually been at my Hampshire home — & gone — & I do not feel as if I had half expressed to you the pleasure of seeing you — or done half to show my pleasure otherwise.

I wish to thank you, on my own behalf, & oldest representative here of botanical study, for your coming to the festival — & to say how sorry I am that I was not able to put it out of my power to do more to show my thankfulness.

Botany has long been, & is cultivated here with no little interest and care, & you have long been the teacher & teachers' teacher alike.

P. S.

I wrote you, asking you to my house, immediately on receipt of my notice from the Secretary, of the time of meeting. But the President of the Assn. had the Hassler before, & wrote before. And above all it was settled he should have the pleasure of introducing you to the Assembly — & he had also the carriages, horses & practical ability in long ride, including the use of all the vehicles. I am right glad that all fell to his lot, & confident he will make it pleasanter for you than any other could. But can you not give us a day, or more?

It is much however to have among us the living Botanist who works for our daily - hourly guides. I hope you will find the day in all respect a pleasant one, despite the gloomy weather.

But is it not possible, to have now for my second governing thought, for you to give one day at least, or more, to a little visit to me before you return? I enjoyed, I need not say, the little go- had opportunity to day last night, — it seemed a strange & novel pleasure — to such degree has my social nature been led me to feel withdrawn from social intercourse — but I fancy it was easier to you than perhaps you expected, and that, were you only with us, you would find it none too great an effort to say more.

A day, two days, three days, if only the weather permits, will, I venture to promise, be made pleasant for you, if you can delay a little, now that you are at my door. Will it not rest you, more even than the South Hadley gathering?

My brother came from England a day or two since, & took my wife to ride yesterday — getting back soon after you left. She was very sorry to miss seeing you, & joins me in all the above. We did not know whether you would come by the early or late train.

Sincerely Yours
Edw. Tuckerman

Amherst
27 Nov. 1875

Dear Doctor

Many thanks for your kindness. I return the Journal at once. The sort of diagnoses given are little better (some of them) than none at all. The Pannaria may be one of the two which I described, but I cannot say which: There is nothing else that I am able to recognize.

My Sandwich notions of species are now of such a sort that I feel but little interest in big new ones — which are indeed rarely described — but as it were Nicked out into the world with a name tied on, so that the ozone may keep a hold on them.

Truly Yours
E W. Tuckerman

Amherst
19 Feb. 1874

Dear Doctor,

Mr Leggett, the editor of the Bulletin, applied to the family of the late Dr Harris to get his MS. papers on cattle to Boards. They hesitated, & wrote to me on the subject. I answered that I felt much as they did, that while I considered that such careful research — so seldom likely to be undertaken — or by so extremely careful a man — ought to be put into print, & the results — work as much now as ever, secured to Botany,

I thanks all for the credence from Silliman!

I should [illegible] it [illegible] to do anything for the publication of Dr Harris's papers, I should be glad.

Sincerely yours
Lew. Tuckerman

Professor Gray.

I could not but feel that a different periodical — say the Proceedings or Transactions of the Academy — or the American Naturalist, were the appropriate place for the paper. And I also said that I would lay the matter before you.

If the m/s. are ready for the press, they are not likely to give much trouble in the printing, I should say.

I thought possibly that you might decide, & designate some fit person to take charge of the paper & see it printed, if you consider that it ought to be.

I am at work on my new book getting on pretty well: though I have to be careful. Field that is my whole occupation, & I fulfil it to the best of my power.

Thank you for your Botanical Contributions of 1876, showing among other things what you were engaged upon at the South last year. I have never forgotten your account of the N. Carolina botanizing so long ago (you will, if to me when I was studying abroad) & have always myself wished to climb those lofty mountains.

Amherst
20 March, 1876

Dear Doctor

I cannot write well to-night, but wish to thank you for your letter. I thought I should lay the matter before you, for what might befall — but did not dream of your doing anything about the papers. Good Dr Hamlin. Nor is it possible for me, away from Harvard Library.

I wrote Miss C. Hamlin that you said Mr. Leggett would make a good Editor — & with that left the matter — and have not heard further, so I suppose I shall not.

I work through the morning

with microscope — I spend
the latter part of the after-
noon in work out of doors,
& the cough is well — being
perfectly well — except the
tendency to fullness of the
head.

So excuse more from

Yours sincerely

Edw. Luckcock

Professor Gray.

Amherst
10 July 1876

Dear Gray

Immediately on receipt of the noble volume of the Calif. Flora, I determined to ascertain the authors of its species, with the following results, which I regard as nearly exhibiting the relative rank in this respect of the authors named, though the figures may not always be exact. I have included also species not yet found in California, but named by you as probably to be expected, and have not reckoned species common to unassociable sections of our country or to foreign countries except W. South America. And in accordance with my view of the true author of a species, I always reckon the original describer as

entitled to the credit of his plant. I find Item as follows:

1,	Gray,	abt 458 sp.
2,	Nuttall,	abt 212 sp.
3,	Bentham,	" 155.
4,	Watson	" 117.
5,	Torrey	" 93.
6,	Torr. & Gray	" 78.
7.	Douglas,	" 67.
8.	Hooker,	" 53.
8.	Hook. & Arn.	" 23.
9.	De Candolle,	" 56.
10.	Engelmann	" 44.
11.	Pursh	" 40.
12.	Lindley	" 29.
13.	Fischer & Mey	" 21.
13'	Meyer	" 17
14.	Kellogg	" 16.
15.	Chamisso	" 10
15.	Eaton	" 10
15.	Durand	" 10
15.	H.B.K.	" 10

with some thirty others who have described less than 10.

How beautiful the volume is to make, one with to go to California, or herborise and I am not sure that I should not do it, had I nothing to keep me at home and busy were my home. Twice what it is.

I hope Jane heal th is good, & that the warm weather is tolerable.

Truly yours

Edw. Tuckerman

Prof Asa Gray

Sorry for my poor Tuckermania. But it cannot be helped — I am well aware.

A. G.

obscure things at the best of us
have no region in which they are
(generally speaking) useful — but
I hope he did not imagine
(I did not) that I questioned
the value of his careful studies:
and I hope he would be
friendly to me in any questions
of system — though he pokes in
much more than I do to the
micrologists. If the study is
to be saved, one must go the
other way, in my conviction.
It is hopeless indeed to say that
"the last results" "the present
tone of study" "the now fashion-
able criteria & characters" are
to be simply accepted with
eyes shut to the past & to
reason. The British Lichenists
may do it (such as they are)
but I will not.

Yours Sincerely
Chas. Darbishire

Auckland
12 Nov. 1877

Dear Doctor

I can well believe
that you have had & have still
exhausting work; no naturalist
I suppose can have more — X
I should not have added to it,
had you not required an an-
swer.

I could not but say what
an honour I should have felt it
to have Sir Jos. Banks's succes-
sor, and so much more a man
but I
too, in my poor house,
I did not dream of it.

In such cases as Wright's
& Lichens; it could not be
but — that there should be not a

few — may make numbers with very few specimens to distribute. In this way the sets differed immensely. I retained all — & my impression is that Wright's set as I called it — was certainly the next in extent, including everything but unique, that the other set had. Wright is, I suppose, your set. The best European set went to the Paris Museum. & this is certainly open to D. Müller. But D. Mitchell look pains to suggest to me that the specimens & wrights coll^n often had been mounted by an ignoramus & that some confusion had occurred — he did not say how much, but implied that the value of the coll^n had been seriously interfered with. I need not say that this annoyed me, and made me wish to go to Kiel.

Perhaps you can hint to D^r Müller that he might go to the fountain head of the determination he is interested in though I cannot tell that I can serve him much. In truth I am not greatly interested in mere Swiss Lichens from the calcareous rocks. They are

botanist, I determined to ask
you about it, for I seem to think
that I sent you something about
Baker's proposal, at your request,
for it's very hard work is it not?
Bird Baker is not here, nor
Lagerheim, nor Cuatrecasas,
for my friend wishes me to
add, nor another cryptogamist
whose name shall not be breathed
by the present lips. And while
these are omitted — the best
persons, left considerable that are
wicked & not to be numbered.
I'm sorry my friend — & I wish
you to be kind enough to believe
me that the discovery
of that & that alone induces me
not to tell you. But I have not tried
a word on your great Jamaica work.
which you must look back at with sad pleasure

ET.

Amherst
29 Oct. 1877

Dear Doctor

I shd have considered
it a great honour if the President
& the Royal Society had called
upon me. It has been my good
fortune to meet, I know not but
all his family except himself — Lord
Sir Wm Hooker presented me to
his father in 1841. I called on
Sir Joseph Hooker in 1872 but he
was absent & I did not feel
well enough to go again. I saw
he visited Prof. Eaton at N. Haven.

Dr Müller has been a correspon-
dent of mine, I would still be, I
suppose, had I not told him that
I could not exchange. I was able to
help him, through another, to many
of our best plants, however this list
is certainly made up, except the little ap-
pendix of "species arcticae" & species finest

published by me. They are from all countries, or tribes — he has therefore either in view a critique of their author studies, or else he simply wishes to complete the material for his own. I suppose the latter without doubt. I always try to do what I can when monographs desire specimens but in this case I must at least postpone action. My specimens are mounted & often very small — I don't think I can do anything in this case — I got a very entirely friendly letter from Dr Müller. He is one of those who regret — to leave the company of microscopists (& pure philosophers) & I do hope he will take a high, independent ground. But — his interest appears to be pretty much engaged in discovering "new species" — in which I cannot always see my way to follow him. There is still as our way to follow him. There is still as our way to follow him. There is still as our

Amherst
17 May

Dear Doctor

Please accept my thanks as well for the Proofs, as for your article on the Jerusalem Artichoke. The latter is most interesting. The fact that the plant grows in Northampton Meadows ~~grows~~ indicates, as you suggest, a northern Indian race, as I take it.

Did you notice that Callitriche is a member of the Amherst Flora, found by my colleague Mr. Frost who writes "Some fifteen or sixteen years ago (1876) in company with Dr. Gray (then of the Wilkes-Exploring Expedition) we

visited the cemetery at Northfield, and while there he pointed out to me the above plant on a little rising patch of ground. There were four rods square. I never had seen a specimen before. I recollect we remarked about the singular locality. There were only a few specimens. I was surprised also to find Arbutus there. "Was" in company with it. 3 days later he adds "I cannot have any doubt as to the plants being a native — no appearance of it's ever being planted, or the ground ever being moved. The plants were sparse and irregularly scattered. The locality was in the then untaken up part of the Cemetery. I do not know of a single circumstance that goes to show only that the plant was wild and in its native locality. D. Gray was an excellent botanist. We discussed the locality and never doubted. We gathered about half a dozen plants each."

I regret this dissatisfactory — but delayed mention. If I could visit the spot. It probably gone now — but it should perhaps occur in the neighborhood as undisturbed soil.

I must indeed trouble you for the MSS. If I could too much — I will pay for the same.

Yours sincerely
Edw. Tuckerman

Amherst
1 May 1877

Dear Doctor

Excuse me if I write but little more, as my head begins to feel sore. I send by Express today the Journal you so kindly lent, and the MS of my paper. There is an Appendix on W Combe's Papers.

I hope you will take the way I have taken his criticisms: if not, please to tell me, tho' that will give you more trouble, which I would avoid.

Have finished three Ch.

Dick, I have kept to determine
no less than 5 parcels &
specimens — & then I go
on with my Handbook,
& Manual, & Synopsis,
& what not.

Yours ever obliged

Edw. Tuckerman

You will perhaps observe that
I have quoted Taylor as the
author of the diagnosis of
"Usnea Taylori J. D. Hook." There
is not the least doubt of
it, having all his little
peculiarities. & Hooker
made & complimented him
by putting his name to it.

Amherst
26 April 1877

Dear Doctor

Many many thanks for the Journal & your note. I have noted the points in M's Cincinnati paper, in an appendix to my paper for the Proceedings — but will do my say something in regard to your kind proposal to notice it by the reverend gentleman.

There was a certain refined way among the old school of English botanists — as not of English botanists at all, with which Mr Blandish accorded. Now it is impossible that a man of M's standing entirely unknown as a systematic

for my revered friend, Fries of Stockholm, a copie d'une fort belle lettre Kotzebuéenne d'indignation & denunciation of him in reply to no end of utterly unprovoked abuse by M. de K — This, through some meanes, Blandish has now found out — & our interesting, which concerned Wright's Lichens, affair, is at an end.

He was happy to have the work to do, but it is now doubtful when we can get the MS. & publish it. The colds he had the coolness, in one of the letters I sent you, to say, as a reason why he should hurry the sending of his MS — that many persons people in Europe wished the Exsiccati & Lichens in America, or if that were an honorable way of leaving the matter, or explaining his course.

I will send my MS written the week on which it was begun. And before the Journal. There is the Excuse for haste & Utus!

Sincerely Yours
E W Tuckerman

botanist except as (I think I may
express it so) the evident mouthpiece
of Nylander — it is impossible he should
adopt the tone he has adopted in
this article to men of so much
greater experience & knowledge as
Churchill Babington (who studied
the Lichens for Hook. H. Britannica) & (perhaps we do add) your
correspondent — if he did not
speak for the Fen Lichenographer
and this a pity that Nylander's
way of doing things should be
grafted into English botany by the
means of this young follower.

A German botany has acquired a
savage tone of controversy which
I have sometimes thought might
be due to the running of protestant
theology with it of religion itself
into the ground there, & the evil
influence of such writers as Vogt
& Haeckel on their disciples — turning
away the land of ideal[ism] into the
[material]
objective — and soaring in the
clouds to wallow in the mud.
But Nylander's tone has always
been the same. I say it is a
pity that Mr Crombie should
not be permitted to use a respectful manner to such a man
as Ch. Babington. He may have made
mistakes it may be: so have others;
but that is not enough to justify
Crombie's writing [Babington] to read.
it would give him pain.
I sent Nylander a parcel of Cuban
decided
lichens some time, with the hope
of some profitable friendly criticism
as he had done most of the work
that had appeared. But he did
not agree with my species, throwing
two into eleven — & added to the
which was pain — the publishing
of them, with his own names, before
communicating with me.
I could not stand it, or his contempt

Amherst
19 April 1877

Dear Doctor

Many many thanks.
the paper
I will send, in a week, I
suppose I may do so, to you.
I did not say, as I should,
that Dr Nylander (with his
immense knowledge, which never
was approached) could not but be
able to criticise me, where everything
almost is open to criticism,
as in the Lichens.
But his present standpoint is a false one — &
he criticises from that position
I hold his criticism of minutiæ,
of no value.

has sent me several of his
papers (but not this).
Nylander
This learned man, lives in a clap
house. I can imagine, but have
not the head now to work
up the [illegible] criticism of
his [illegible] of species-making.
Theodore finds it dry, I often
my Scandinavian Lichens go, in
the course of his Lich. Scand.,
& a curious exhibit it makes,
considering that this is not a
humble study of nature in the
field who is the object of the
criticism but the arrogant
authority of lichenologists; who
cannot bear any difference
from themselves.
May I send you his two
last letters. Yours ever
Edw Tuckerman

The letters can be slipped into the
pamphlet.

For instance: I incline to think that while one salient point may indicate a species so-called — all other points should coincide so far as possible to the assumed distinction: or if they do not, if in all other points than the one noted the plant agrees plainly with something else — I do not think there is good reason for separating it. Mr Lauder most commonly takes the one point of difference (be it the chemical reaction so far as obs'd, or a little diversity in the spore-measurements, H. F.) as enough, & his new species rest, by scored, or such criteria. They are not species: or "species" are hardly worthy of our serious study; as the Germans, many of them, think. I ventured to say, in a letter of mine that these observations (of the reactions, the spore-sizes H.) had their value of course! but that their systematic value was only a matter of opinion. But I expressed this in my haste, so that he understood me to refer to the value generally of his observations as only a matter of opinion: I take this to be what he ironically apreté: and which, with the discovery of my treason in adopting this word about him broke down his patience.

No doubt — he can criticise me. Perhaps I ought to see the article, as I am to print, & you kindly permit me to ask you for it. Mr! Combie

Rochester
17 April 1877

Dear Doctor

Mr. Crombie is only the English Secretary of Dr. Sylander (Leighton having been deposed from that high position) & it is Sylander & not the other Rhode criticism you refer to. Sylander was criticised by me in the article, as I took the view that two plants which he put in different genera were one and the same in fact — & that his diagnoses even showed no difference. And he is especially angry because he has ascertained that in a private letter written by me to I know not which of my correspondents — sometime about 1856, I quoted with my approbation

To him every thing that comes along now appears to look like a new species — that is the fact, which to me it is the last supposition. I distinctly declare that the larger parts of these (named) species have not even sufficient characters to stand on there is not even prima facie evidence of their distinctness. And yet if I name a lichen by the name of the larger species which would you take it in, & neglect the Sylandrian mere names, I shall be condemned.

I am wholly sick of it, & feel less & less respect for what the Germans call Systematik — & indeed it is in such a state as Sylander — & the German heads are in, hardly to be wondered at. Anatomists hardly look at it, & so I suspect the French, for he can't get into the annals now.

Really I can hardly but find limit to the leakage of blood to the head. I have a paper nearly ready which I should like to get the Academy to print (in proceedings) if possible.

Truly yours
Edw. Tuckerman

some severe remarks of the German D. Koerber on himself. Taking this in connection with some letter Carruel appears to have to send me the rest of the determinations of Bright Lichens he has now withdrawn himself from me (I suppose) entirely. As there is hardly a page of my book on the Flora Scheda is not a criticism (though always respectful) of his views — he has now quite enough to exercise his versatile mind, & probably will do so.

But it is very odd that he should have got hold of that private letter! I wrote immediately to Koerber himself to enquire if it was to him I had written, & he says no — of course I can now only guess. But the letter of Ben Lowe's was written when I was annoyed by his cavalier treatment of my Cuba decided (that is of a part of them, in fact only a small part, in respect to which I still hold my own views) is his excuse for violating his engagement now to long protracted to finish Bright Lichen. I have got right to write & have sent him with another appeal, but have no answer yet.

I never see the Journal of Botany & I do not expect very soon to go to Norton: but I assure you that anything I may do in determinations of Lichens (though I have had 30 years of experience since my determinations passed pretty well the ordeal of Acharius &c.) will be exposed to contradiction from D. Mylander.

Amherst
9 May 1878

Dear Doctor

Please direct a
copy of your Flora to be sent
to me — for which I enclose
payment. It seems wonderful to me the work
you carry through!

I am busy now occupied
with my book of Species
of N. A. Lichens — and
doing nothing else. You know

Let me also thank you for
the last no of your
Botanical Contributions

Sincerely Yours
W. Tuckerman

how precarious the difficulties are in view of the fact that the very conception of species has been eliminated from the mind of the man who knows most about Lichen-forms — and how impossible it is to hurry — but I go on steadily.

What students first study are the Parmeliaceae — Lichens embracing one half of the whole of the department. I propose to publish them separately when done.

Mr. Watson wrote to me to make a Lichen-Flora of California to be appended to his Flora, & asked my advice. I gave it to the effect that such labour would be far less use to students than the whole book that I am straining every nerve to finish, while Lichenists would find a list exactly as useful, & offered this list. I feel sure that, under the circumstances, you cannot but approve my action. This side-work, while I am so busy on the main work would be too much for me, & of no use, to speak of, to anybody.

Amherst
14 May. 1878

Dear Doctor

It is too long
since I received the pleasant
note, coming out of the midst
of your distractions, which I
can imagine: but I avoid con-
tinuous writing as much as I
can, though the interrupted
work of study is easier for me.

Last december changed, for the
first time, the first figure of my
age from 5 to 6, so that I
decline to parade with the vet-
erans yet awhile — and, were
it not too deafness, and the
many to be studied: and confess,
that there is nothing fit to be called
Science, beside Anatomy generally
(I mean of course of Plants.)
If it is to that, that the French author
is bringing the study. Even now
a German Lieheadologist, one of his
own followers, complains to me
that the single genus Lecidea
as represented in Europe alone,
has become a maze to which he
has not can gain no clew!

What can I say of your great
work! Nothing — only that what
you have done gives a promise of
what you will yet do — complete
the labours of your life! May
your Health & Strength be sustained
to the End. Believe yours
Edw. Tuckerman

tendency to congestive troubles, which deprive me of tobacco & tea, & make case requisite in all study — I should com more distinctly declare the honours of age. If indeed my calling kept me constantly in the open air & on foot, I do not know that I sh^d have much to complain of except the isolation for which there is no cure. But — except all that which was provoked by your letter.

My wife says she can't remember when I have worked so steadily as this summer; and this may shew you, that I am busy with the book that is desired. But let me say that if I could do as Mess^rs Leighton & Combe are doing for the country & a Borrer & a Greville — that is to say accept & adopt implicitly all the "new species" which Dr Hy. Landers will be engaged in pouring forth, & being in a new country instead of an old one, add to the above a host of other like "new species" of the approved sort — providing regularly worded & spaced diagnoses, with micrometrical measurements, H. N. — the book w^d be easier than the one I am on, & would pay master generally not only here, but I fear also abroad: and yet I do not think that it would be a worthy object of a man's labour or a fit guide for students. Better far renounce the system, except as furnishing tags for speci-

Amherst
24 Aug. 1870

Dear Doctor

Thank you for your kind invitation, & please tell Mrs Gray, with our united regards that my wife joins me in wishing that we may yet have time and opportunity of it. But just now & for some time, we have anxious friends with us.

I have seen but one fasciculus of Wright's Lich. Cub. This includes all the Tribe, but the Graphidaceæ & Verrucariaceæ. The former is small in species except in the Corticiei regions of the earth — but has received here enormous expansion principally due to Mr Nylander. No one can work up this tribe without constant consultation of the great Europ[ea]n collections — and I am compelled to refer it to Mr N.

The other Tribe has also its principal

Dr Müller has a deal of independence. I have just written to him — and should like the credit & handing over the collections to him — though I wish you would communicate with him yourself if you approve, and arrange the practical part & the business. I said I should like the credit & the thing because he is the only friendly correspondent I now can look to for help in Europe: those who are Mr Lindsay's own have given us kindness. Themselves utterly — with no other cause whatever but my copying the remarks of Kitr, perhaps with some little introduction, in my letter to the other Europeans, who, for their own reasons shunned it to N — & beside that my differences from the latter on to System.

To make sure a good deal of intercourse will be needed, even for the part which he has determined. Yr M. can do it.

Yours sincerely
Edw. Tuckerman

developt in tropical & subtropical regions — and, though more manageable here than the Scrophulariaceæ, it could hardly be elaborate before the other — rather it should hardly have been published before it. So I sent this disc to Sclater. — He engaged shortly to give his attention to the book & finish it up. The French war hindered, but he worked on it well for a time — & I have his MS. determinations of all he has accomplished.

Vexed by his conduct with regard to the plant elaborated by me (I send — his very discredited speech before they could appear here, & he chose to publish them — among other things making 11 out of 2 species of mine — which I still hold to constitute (not 2 species) — and in writing another European correspondent, general, I quoted entire a rather just criticism of the plan by a German author. This, he wrote me, he had got hold of; & informed me that it was "awkward"; especially so no doubt in view of my difference with him generally in my book on species. He desires to finish. Now I wish to propose that the whole material remain, with Sclater, till I determined then, be sent to Dr. J. Müller Arg., to be by him put into order & determined so far as needed. He is strong & enthusiastic. The coll[n] is so large that he can hardly expect a better chance to secure for himself the most difficult Tropical Lichens, determined too in good part by the most learned writer on the Order in question, & whose determinations are essential in all study of the Order. If you approve the plan, I will defray the pay all expenses — transportation, paper & printing. I see no other way of getting these lichens into the hands of students. I cannot do more myself.

Flora, his "additamenta ad Lichenographiam Europaeam (so well studied already) in each of which very short descr^ns of from 20–50 new species are set up — with no explanatory remarks or reply to mine. If these little descr^ns are analyzed — no difference of any importance will be found noted, from older species. And one is wholly inclined to agree with the writer in his final criticism on his work, which most correctly follows, "vix species". Just notwithstanding that, the "vix species" is always taken up when next it comes in view without the vix, & if it appears in any Flora, it has the n^o and place of the older species. But what made him ever separate it? A little differing nuance of colour, a proportionally differing magnitude of fruit, rather longer or wider spores, or especially a difference in some chemical reaction or failure to react — either one or all of these suggestions, as seen in a common Lecanora, his "additamenta ad Lichenographiam Europaeam (so well studied already) in each of which very short descr^ns of from 20–50 new species are set up — with no explanatory remarks or reply to mine. If these little descr^ns are analyzed — no difference of any importance will be found noted, from older species. And one is wholly inclined to agree with the writer in his final criticism on his work, which most correctly follows, "vix species". Just notwithstanding that, the "vix species" is always taken up when next it comes in view without the vix, & if it appears in any Flora, it has the n^o and place of the older species. But what made him ever separate it? A little differing nuance of colour, a proportionally differing magnitude of fruit, rather longer or wider spores, or especially a difference in some chemical reaction or failure to react — either one or all of these suggestions, as seen in a common Lecanora, Excuse my writing. It tries my head to write continuously — & the hand unbeknown. 109

Amherst
2 Dec. 1878

Dear Doctor

I wrote to beg instructions how to despatch the box of Wright's Lichens to Miller. The case is 3ft 2in long, 1ft 8in wide & about the same height. W. Wright is happy to have it so.

I have never sent anything to the rest except Wright bookwise, or the Smithsonian — neither of which is now available.

If you will kindly direct me, I will at once despatch the box.

I have been engaging a stout cart, [illegible] & partly

considering foreign work for which the season has been so favourable. But I expect soon to take up my work again on the Lichens, & to stick to it till enough is done to enable me to bring out my 2nd part (Parmeliacei) which is just one half of the whole.

I have long sought to express to you with what reverence (I have no other word) I read your "Forest Geography & Archaeology". In these days of species-peddling (species = Krämerei as the Germans have it) which is about all that the more active lichenologists proper are engaged in, one sometimes inclines to question whether his study is worth the pursuit (I mean the systematic part) — but such botanists as you & Dr Hooker maintain the rank of the science.

You may have been surprised at my saying in my last, that I could have offered to the world a great many "species" of Lichens which I do not believe to have any claim to the rank — & intimating further that in this I should only be following the example of Paris. This seems to imply misconception on my part. What I mean is this. The units referred to is in the habit of sending out at short intervals, in the September

single specimen, or from a single station
& sent to him by the finder. —
Now a man may (not with standing evi-
dent species-greed which has always
been a marked feature of the author)
satisfy his conscience for the worse
for the apparent closing of his eyes
to all through comparison with affinities,
by an "it is best so" — capped by his
final "six species" — but what are
we to say when the poor fiction
comes up again & the author simply
drops his "six" & troubles himself
no more about him, barbering except
to express respect for "L. from his
fellows, who (Mr. Crombé &c)
implicitly pay it.
I say then, that if I chose to
be a fellow of the Parisian
doctor, & to make the most of
any difference in the kitchen of
this great continent of North
America, I should expect to be

able to publish ten times as many valuable things. In little day notes, words like "so-and-so" could be easily filled out — there is nothing in them that is not true, or meant to be, were

I wish, would to God, that the scientific judgment which alone suffices to give value. Nothing but that!!

It is well then that the Medium amabile (it will always be that) should be shewn, and that not only by the mere numbers, to be a Science.

I pray for full direction as to the sending the box, having little to do now with business people. There is a Foreign Express in Boston, & a Jno Richard & Bros in N.Y. but I know little of them.

Sincerely Yours
Edw. Tuckerman

Amherst
15 Feb. 1879

Dear Doctor

I received the 2d of the St. Petersburgh Acad. Bulletin, which you so kindly sent me & immediately cut it, & turned it to day.

I do not profess to know much, because I in fact know but little indeed of French; it strikes me however that the Professor's French is rather peculiar.

As to his paper I can only speak with the respect due to such a contribution to our knowledge — but I do not see that he gives anything beyond a history of the development of "Nostoc" from the gonidia contained in the cephalodia of a lichen. And suggesting that his precautions to render it (as far as he could) impossible, the entering into the organism of foreign elements (the chief difficulty in all attempts to decide by cultivation) were quite sufficient, & that "Nostoc" was really seen to be a product of the "gonidia" down, what does that prove? Not the hypothesis of

I remember, that I can see. Will any cryptogamic botanist be willing to pronounce confidently on the gelatinous place of Nostoc commune? Is not the hypothesis that it is a certain state of the Lichen Thallus, or Glaucelle or any other?! And is it not simply an assumption that in the case in hand we have an instance of the parasitism of a fungus organism on the Lichen. I venture to ask also (in great ignorance) whether there be no assumption in marking off so very distinctly the blue gonidia "colorées par la phycochrom" (the collegonidia I say sooth) & the yellow-green ones "colorées par la chlorophyll" — and further, stating that the former "belong consequently to the Phycochromacees Algæ, & the latter to the Chlorophyllacees." For myself I know only to depend on what I have seen of the Lichen structure in question — but it leads me to the doubt I have expressed. And further, if the reasoning & the facts) be valid, the Algæ in question will all require to be studied anew, with the result, it seems probable, of lessening if not of putting an end to their separation for Lichen-growth.

But granted that, in the case of (assumed) foreign element — the "Nostoc" for instance — finding themselves on the fronds of Peltigera aphthosa, the hyphæ of the latter (that is, in this case the little hair-like filaments of the upper surface) should amicably enter into certain relations with the "nostoc" in question & that intercourse should end in certain new thalli in which, while they agree in almost all of their respect with the parent-thallus — the gonidia of the latter should have their place taken by what seem a modification of "Nostoc" — do the "cephalodia" in question enable us to look beyond themselves?

Excuse haste, as I have written as long as I well can.

Sincerely yours

W. Tuckerman

lodgings at Berlin to tell me of Kunth's unhappy death! Now in April 1851 the kind hearted man and genial botanist is himself gone, (of apoplexy.) Wahlenberg was an Octogenarian. I made him a very agreable visit in 1842, and have since exchanged parcels with him. In June 1850 I saw him again, scarcely altered as I thought, the very picture of strong old age, white his scarcely changed hair, & dark complexion so much concealed that if it had not been for a trembly motion, one would hardly suppose that this was a man who acquired his celebrity during the first ten years of the present century.

Both were old Bachelors, and I shall not soon forget the pleasant "Bachelor's Walls" they maintained. Wahlenberg's establishment was in the Garden House of the old Botanic Garden of Upsal — and was a little Paradise to my Eyes. He gave me a very large set of his Polarplanten in 1850 — but I was so unfortunate as to lose the parcel on my way back. This has promised to make it good however. My health prevented me from doing what I wished. Set down to this only that I did not succeed in supplying more of your desiderata —

Truly yours

Edw. Tuckerman.

P.S. I send also a small parcel, (marked for you) which I found among Mr. Oakes's Lichens.

P.S. I must deny myself, for the present, the pleasure of calling at your house, though I shall hope to at some future day. Please remember that I am no longer the inhabitant of one room but of a considerable House, in Cambridge, when you are here next, & shall be most happy to see you.

Cambridge 7 July 1851

Dear Sir

Your welcome note of the 28th ult°, reminded me that I had not sent you the European Potamogetons — and also that there were still American ones with which I could supply you. I send herewith these for the Europ[ea]n & Americans — having gone through my whole stock of the latter & put up all my duplicates of the former, which I hope you will not find more of a burden than I have. The truth is that I have neither time nor disposition to keep up any system of Exchange — and have been doing my best therefore to get rid of a stock (of Duplicates) which as it was made in Dr Oakes's school, was enormously large.

For the present I limit my studies & collections strictly to Potamogetons and Lichens — not having time enough even for

these: as witness an Enum^n of the Lichens of the Southern States, for wch I have very large collections, and finished the manuscript of — up to the descriptions & insertion of the new species last November — & yet have not been able to touch pen. I shall not be able to travel any this season for Potamogetons — & believe I have exhausted the species in this vicinity.

Whatever you are able to do in this field will have great interest for me. I think we ought to have P. obtusifolius here — but believe that no Amer^n botanist has yet seen it. The species is indeed inserted by Gray in his Manual of Botany, I believe — but I know not on what authority. Certainly this herb^m did not appear to me to contain the plant. I should have said P. pusillus — was what you sent me, but I did not looked at the specimens, when I wrote for so long — that I forgot. But I fear I spoke too fast above — when I said I had exhausted the Potamog^s of the immediate neighborhood. I doubt whether I have, though I have looked pretty carefully.

Pennsylvania, New Jersey, & Delaware I expect novelties from. Indeed I have seen good specimens of P. crispus, sent from Delaware by the late D^r Griffiths to a German Botanist. The only American authority for this saxon species was Torrey's old Flora of the U.S. — where the species is incorrectly inserted as from Lake George. I have not only examined the waters of Lake George, with this citation in view, but have since seen the original specimens (as I suppose) in D^r. Torrey's herb^m, from which the name crispus had been erased. Schweinitz's authority is given for P. decipiens as a Pennsylvanian plant, and he could hardly have mistaken this extraordinary species.

I should be delighted with any species or notes with which you may be able to favour me. I never saw so fine specimens anywhere as some of yours; and have not myself the art of preparing such. When the fruit is perfectly ripe, I find no difficulty in determining the main characters from it in a dried state, without extraordinary trouble being taken to preserve it — but should always choose to prepare a full description of it in the recent state.

I have been pained with hearing of the death of two eminent Botanists, who were kind friends of mine — Wahlenberg of Upsal, and Kunze of Leipsig. I saw Kunze this 2^d visit of mine, in April 1850 — & it was just at that time that he came to my

Amherst
13 Jan. 1881

Dear Sir

In your note referring to the 2nd Vol. of the California Flora which I congratulate you on having completed, you remark that I "shall not regret the omission of the Lichenes." That is quite true — but not because I allow that the genera and species of my list are only "reputed," as you call them in your Introduction. There is no reason at all for that word except as applicable generally to the work. And I do not think you suppose that

you intended the word to have the only sense that I can yet give it. A friend of mine in Cambridge who professes all Thallophytic Botany may well be puzzled, & inclined to think his whole province in flux — but I do not regard the condition of any part of it as at all chaotic at present: whether or no it be to receive large accessions of supposed Fungi in future.

But I congratulate you once more on the accomplishment of your arduous tasks. Pity only that you could not have avoided the heavy paper so affected by American publishers at present. That makes all the handsome books so difficult to use, that I wonder that Cambridge accepts it.

The Specimen from Mr Macowan arrived safely.

In haste truly yours

Wm. Thiselton

Lereno Watson Esq.

Amherst
8 Mar. 1882

Dear Doctor

I had the pleasure to receive your note this morning, and send you at once a copy of my Synopsis. It was my preference to publish it myself, & avoid all adverse criticism & notice in print, as I did with my Genera, but the Boston publishers in some way got an unknown hand of the book, and the fact that it was considered for me at

I think you must gain immensely by your frequent voyages, and hard labour lately in the interests. I am myself well, and so is Mrs Tuckerman.

She would gladly join me, if in, in kind remembrance to Mrs Gray & yourself.

Yours truly
Edw. Tuckerman

Now Yeni to have in pay the pointer — (and no other) led me to give him the work. I only stipulated for ten copies and sent them at once to my fellow-Lichenist. I have since procured ten more, & am glad to offer you one.

It is a great annoyance to me to have to issue this separate of my opinion, because I stand alone to be considerable an extent, and differ especially from one writer, who is noted especially for the moderateness of his largely subjective (and therefore unscientific) judgments, & general bad temper in face of opposition. I am now a little more used to my book, and feel willing that Prof. Barlow shd notice it.

The next or other part will involve all this disagreable contention & to a greater extent, (as see the note on Lecidea, at the end of my introduction) and I may find it too much for me — as specimens may be needed which I cannot expect to obtain — but I shall do what I can to complete the hand-book.

I must decline, and keep a humble position.

Truly yours
Edw Tuckerman

Amherst
13 March 1882

Dear Doctor

You are kind to think of sending the book to Sir Joseph Hooker. I shall be very glad to have it reach him.

I confess, if I have not already, that I did not mean at first to offer my Synopsis to any body who would be likely to notice it in print.

Your writings are so voluminous that you can hardly feel as I do, nor do I know

that you can be exposed to morose criticism of your scientific judgments — My different position deprived me of all pleasure when the book reached me, & I did not even show it at home. It was impossible for me to publish at all without freely expressing my opinion on the merits of the most prominent publications of the day on lichens, and this not only in the synonymy of the groups, but more at large in the introduction; but I could not do it with any comfort. I am now a little more easy, because thoroughly satisfied that I am right.

I enclose the address as you desire.

I decline to be reckoned among the Patres. It is not my habit of character at all. Studious is my rank. I have just got a letter from Dr. Michener (Toughkenamon, Chester co, Pa.) in which he says, sending a specimen, "I am now an old man (88) and cannot do much work, but the interest remains." That indicates the criterion of a Father! If you like to be included, I cannot have anything to say — but

Cambridge 11 Feb. 1853.

Dear Sir

You found me the other day in the midst of the dirty work of getting my herbar to rights — I regretted however not only this — but still more that there was no presiding deity in my house to make it more agreable to you during your stay. I hope for better things some day, which is enough on that score. But I fear I must now prepare you for being overwhelmed, nolens volens with a small ocean of dried plants — which I should have burnt if I had not determined to send them, at all risks to you, and which you can burn now if you will not have them.

Last Spring, agreable to promise, I sent out to Prof. Reis my whole phanerog. Herb^m & Peru — and also my large collection of Carices of every part of the world to D^r Boott. There still remained however (in my stable) a large trunk of duplicates, besides sundry miscell^s piles of plants, which the damps and Spring Fleas were surely consuming. I made up my mind to be rid of them — & to distribute the dupl. of Carex to D^r Sartwell — & the miscell. foreign plants to D^r Dallington who told me in December they would be acceptable to him. Among these were two parcels (the largest I ever received at one time) of European plants from Reichenbach of Dresden, which had been lying undistrib-

bed ever since they arrived in 1844.

Wishing to know what was in them, I opened them & ran through the whole. A happy thought occurred to me which accounts for my sending the large parcel I referred to to you — The thought was to pick out everything of interest to a New England botanist — all the Alpine plants in which the collⁿ was rich — & all those species which are common to us and Europe — & send them to — once it would have been Oakes — once again it would have been I who would have prized them — but now there remains only yourself as a real New England botanist — & so to you I determined to despatch them.

Besides the foreign plants some considerable remains of my stock of N. E. plants remained — which I could not destroy while there was a chance that you would take them — though I have destroyed as many perhaps as I have sent you.

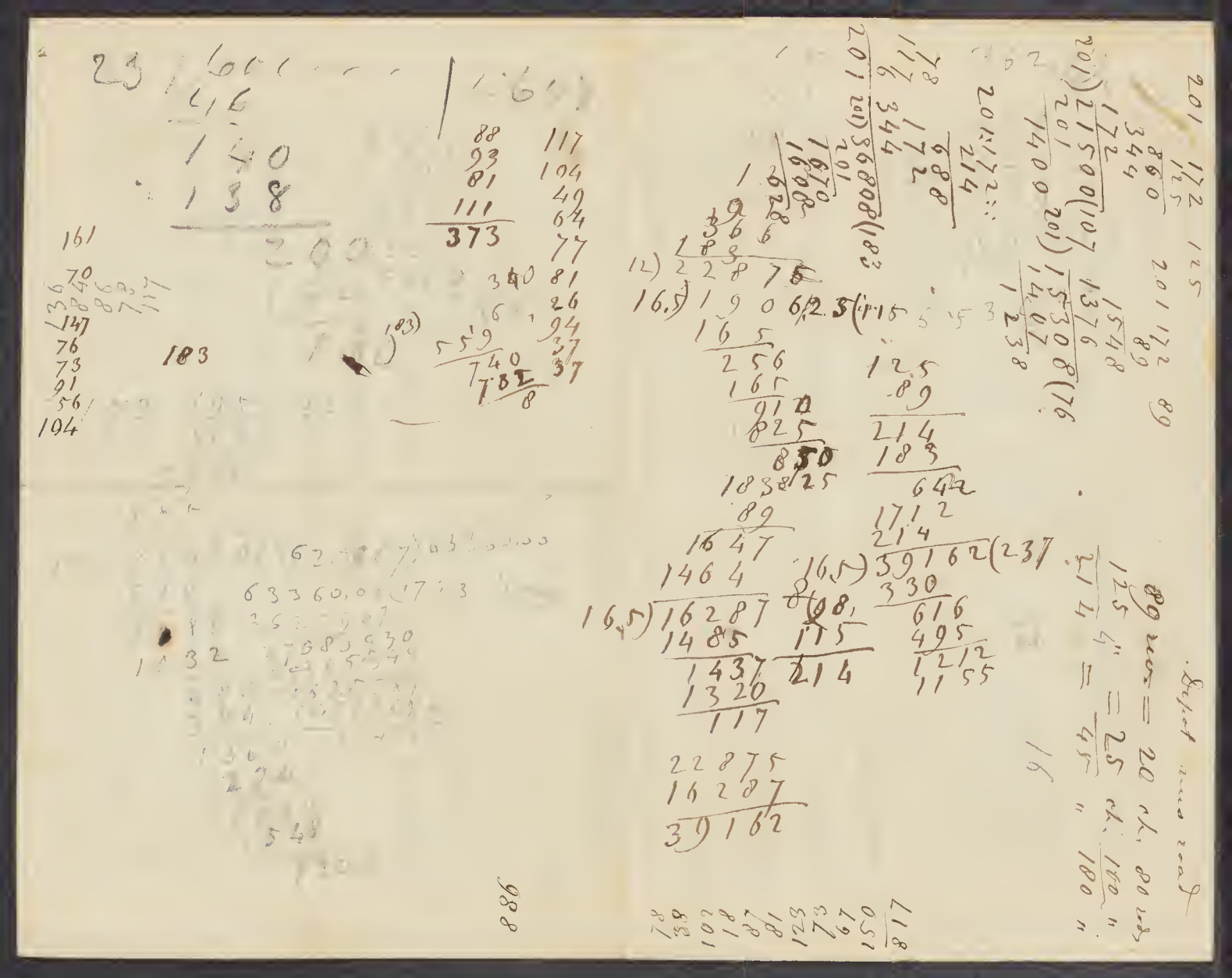
Then you have the unvarnished tale of this business. Will you not accept of the parcel as a token of my conviction that you ought to have them, by clear right. Once more I assure you I should have destroyed the whole if you, Sartwell & Darlington could not have assumed the burden of them.

Among the foreign plants are good specimens of Ulric. vulgaris — derived by you; and among the Amer[n] are some rare N. E. plants — as Bigelow's Viola acuta from his Station, now destroyed — V. pironia found by me in Cambridge — Dentaria diphylla from its only locality in Eastⁿ Mass. I believe, West Cambridge — now also no more to be found — ~~Aspidium~~ Polypodium Dryopteris from the Camb[e] Station now also gone — & & — which I am delighted to send you — having regretted often that I had not excepted them from my herb[m] when I sent it to Frei — & not knowing that I had any duplicates.

The plants will be forwarded on Saturday or Monday & I trust you will like them Yours truly

Tho[s] Nuttall

Rog[s] Robbins
Cambridge



Amherst
17 March 1884

Dear Doctor

I think your judgment should at once decide the question as to the successor to Professor Hees among the foreign Honorary members of the Academy, and I have really no opinion to offer, nor any right to one, Messrs. Laforte, Eichler, & Maximowicz being quite beyond the

large of my knowledge.

I am continuing to do my best with the determination, and characterization of our Lichens, and am content therefore to leave the results to future students to make what they can of — sure that whatever mistakes they find, they will themselves be found to have made perhaps as many more.

I sometimes think that a superior being, looking down with competent eye on much of the systematic labours of our time (I mean of course in one little field) would find terms like "science" & "scientific" inappropriate in such relations; but we must do what we can, and reach our reward in furnishing Indexes, the value of which, to the philosophical investigator of vegetable structure as to the humbler but not less happy student of Habit, and Special Morphology, is alike unquestionable.

I am Dear Sir Yours

Edw. Tuckerman

Prof. Gray.

Amherst
29 May, 1884

Dear Doctor

Please accept my thanks for the offer of Fries's Flora Scanica but I have the book. Nor do I think of any one who would be likely to desire it. But the time will surely come when the systematic genius of Fries will be more instructive than ever before; and a new "Reformation" of the system as the microscopists have left it, make all things new. Good official Deus!

Truly Yours
Wm Tuckerman

Amherst
21 Oct. 1885

Dear Doctor

I do not see how I can help you in the matter to which you call my attention.

The reference in Mr. Müller's letter to my own herbarium as, after my death "to go in the herbarium of Harv. Univ. (if I am not mistaken)" is quite enigmatical, as no such appropriation of the former was ever made, or seriously entertained by me.

Excuse my saying any more upon this matter.

I am very truly
Edw. Tuckerman

Encouraging, is it not?

Dear Doctor

I had write immediately to Mr Wyatt, & ask him how the work upon his collections goes on.
I have not heard from him for a good while, — but as soon as I do, it will give me pleasure to carry out his wishes as fully as I can.

What now remains can be despatched much more quickly abroad than here, but I am unable to say at present how soon such foreign elaboration can be undertaken, as the only botanist fully prepared for rapid work of the kind, (Prof. Oglander) has his hands entirely full at present with his own most important work, besides academical duties in the winter.

That Mr. Oglander has kept me waiting, now more than two years, for his determination of some two dozen

of Wrights' Oceanic lichens, (specimens that were especially desired to add to my poor stock of types of the great family of tropical lichens which I have in hand this winter) I should desire to get good assurance of his willingness to do the work immediately, before committing it to his hands.

I sent a copy of the last paper printed for the Proceedings (but not as yet published, so far as I am informed) to Mr Wright, as soon as I received the copies. I shall be glad to replace the copy you have now sent him.

We have resumed to-day for the winter, not returning to Cambridge before the first of May.

Accept E. Tuckerman.

Amherst, 25th Mar. '86

My dear Sister Kay,

I have not been able to use my eyes for some weeks past but I feel as if I could not let another day pass without telling you how grateful to my heart were the kind words you sent me after your return home from Amherst.

Knowing my dear husband's high esteem & regard for you, and the

would take the heart out of what he was doing.

Since last September, he has been very far from well, and in Jan. his eyes were involved by the disease, but notwithstanding my solicitude, I did not dream that he could be called to his rest so soon. He suffered from weakness, but had little acute pain, and I have the great comfort of knowing that he was spared the distress that he would probably have had to endure had life been prolonged.

Again thanking you for your kind sympathy for me, in my great sorrow, I remain

Very Sincerely Yrs

E. L. Huckleman.

pleasure he often expressed in your long and unbroken friendship, it was doubly gratifying to read the testimony of such an old and true friend to his noble character and attainments.

The attack of confusion of the base of the brain which Mr Sherman suffered from, thirteen years ago, greatly increased his deafness, and the sensitiveness of the nervous system to anything unusual or exciting in the regular routine of his life, so that he had to keep society more and more and withdraw himself into his library, where surrounded by his books, his harmonium and his kittens, he devoted himself wholly to his studies. I can confess now to the great upset with which I saw him obliged to keep more closely at home, and the constant anxiety I felt about his health, and the fear that he might overdo. After entering Yale, he brought out the first part of his N. Am. Lichens, but since then, though working steadily when he could, in the second part, I could not but see that his bodily strength was failing. He was disappointed too, that his work did not meet the recognition that he felt that it deserved in Europe, and this depressed him, and at times